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ABSTRACT

Issues and themes in the contemporary social sciences are explored in a collection of papers by South African scholars. Thirty-five papers are organized by 10 themes. The first theme, metatheory in the social sciences, includes: "Beyond Relativism in the Natural and Social Sciences" (M. Hesse); and "A Critique of Bhaskar's Transformational Model of Social Activity" (R. Miller). The second theme, which covers recent trends in specific disciplines, includes: "Recent Trends in Psychological Methodology" (P. du Preez). The third theme, interpretation and rationality, includes: "Linguistic Models and Hermeneutics Methods: Rethinking Dilthey through Gadamer and Ricoeur" (T. Louw). The fourth theme, morality and ethics in the practice of social research, includes: "State Censorship, Research in the Human Sciences and Library Resources in South Africa" (C. Merrett). The fifth theme, cultural and media studies, includes: "Paradigms in Media Research" (P. Fourie). The sixth theme, discourses and power, includes: "Is Foucault's Project a Nihilistic One?" (C. Frigerio); and "Discourses on Childhood Sexual Abuse" (A. Levett). The seventh theme phenomenological and other qualitative views, includes: "Research and Reality: How Clinical Theory and Practice are Actually Developed--Case Study Method in Cognitive Behaviour Therapy" (D. Edwards); and "There's Method in Our Madness: A Deconstruction of the Paradigm Problem through Psychopathology" (S. O'Dowd). The eighth theme, issues of conceptualization in the study of government and law, includes: "Theoretical Approaches to the Study of State Bureaucracies: Some Preliminary Distinctions" (D. Atkinson); and "Rights-relativism in Afrikanerdom: Van Warmelo's Sociologism" (J. Hund). The ninth theme, understanding and explanation in anthropology, includes: "Understanding Other Societies through Anthropology" (P. A. Erasmus). The 10th theme, paradigmatic shifts in geography, planning theory and archaeology, includes: "Method in Archaeology: The Selfish Gene in the Prehistoric Mediterranean" (Z. van Straaten). (DB)

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Johann Mouton and Dian Joubert

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Series Editor: Johann Mouton

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KNOWLEDGE AND METHOD IN THE HUMAN SCIENCES

Edited and introduced by

Johann Mouton and Dian Joubert

Human Sciences Research Council, Pretoria

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CONTENTS

List of Authors x
Introduction: Methodology in a (post/anti)modernist era 1

Metatheory in the social sciences

1. Mary Hesse - Beyond relativism in the natural and social sciences 13
2. Ronald Miller - A critique of Bhaskar's transformational model of social activity 26
3. Johann Mouton - On values and objectivity in social science 39

Recent trends in specific disciplines

4. Peter du Preez - Recent trends in psychological methodology 57
5. Dian Joubert - Towards a human sociology 76
6. Wally Morrow - Recent trends in educational methodology in South Africa: Are there any? 95
7. Robert Thornton - Recent trends in the study of culture 114

Interpretation and rationality

8. Tobie Louw - Linguistic models and hermeneutic methods: Rethinking Dilthey through Gadamer and Ricoeur 139
9. L. Peeters - Why do we need interpretation? 156
10. David Sapire - Jarvie on rationality and the unity of mankind 168
11. Anton van Niekerk - To follow a rule or to rule what should follow? Rationality and judgment in the human sciences 179
12. Willem S. Vorster - On presuppositions and the historical study of the Jewishness of Jesus 195

Morality and ethics in the practice of social research

13. Christopher Merrett - State censorship, research in the human sciences and library resources in South Africa 215
14. Eleanor Preston-Whyte - Research ethics in the social sciences 239
15. G.A. Rauche - The relationship between science (theory) and morality (practice) 260

Cultural and media studies

16. Pieter Fourie - Paradigms in media research 273
17. P. Eric Louw - Analyses of cultural phenomena like the "alternative media" in South Africa 286
18. Johan Muller and Keyan Tomaselli - Becoming appropriately modern: Towards a genealogy of cultural studies in South Africa 301

Discourses and power

19. Carlo Frigerio - Is Foucault's project a nihilistic one? 323
20. Amanda Kottler - Contradictory discourses: An analysis of a male/female conversation 331
21. Ann Levett - Discourses on childhood sexual abuse 343

The phenomenological and other qualitative views

22. David J.A. Edwards - Research and reality: How clinical theory and practice are actually developed - Case study method in cognitive behaviour therapy 359
23. Fidéla Fouché - The phenomenological reduction and its relevance to human science and ideology critique 375
24. Trevor Hoek - Awareness of the self in relation to others: A discussion of the method and findings of an empirical phenomenological study 385
25. Dreyer Kruger - The question of knowing and truth in psychology and psychotherapy 396
26. Sandy O' Dowd - There's method in our madness: A deconstruction of the paradigm problem through psychopathology 407

Issues of conceptualization in the study of government and law

27. Doreen Atkinson - Theoretical approaches to the study of state bureaucracies: Some preliminary distinctions 419
28. John Hund - Rights-relativism in Afrikanerdom: Van Warmelo's sociologism 432
29. T.P. van Reenen - Paradigms in comparative law: A critique 444

Understanding and explanation in anthropology

30. P. A.Erasmus - Understanding other societies through anthropology 463
31. Kees van der Waal - Micro and macro levels of analysis: The "unit of study", a non-problem in anthropology 477

Paradigmatic shifts in geography, planning theory and archaeology

32. W.S. Barnard - Diverging geography and the radical contribution 493
33. John Muller - Planning methodology: Empiricism, rationalism and the practical imperative 509
34. Mary Hesse - Archaeology and the science of the concrete 526
35. Zak van Straaten - Method in archaeology: The selfish gene in the prehistoric Mediterranean 535

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Introduction: Methodology in a (post/anti)modernist era

1. The search for the lost paradigm

Perhaps the most obvious observation that one could make about the current state of affairs in the philosophy and methodology of the social sciences is that we are between paradigms. Even a cursory reading of articles and books in these fields reveals a plethora of viewpoints and frameworks and the lack of a single, unifying paradigm. Compared to the scene in the fifties and early sixties, the boundaries between different points of view are constantly changing. For example, not only do we have a hermeneutic movement, but we have critical hermeneutics, depth hermeneutics, a hermeneutics of suspicion and of faith, etc. Realism has branched into moderate, critical, Marxist, convergent, theoretical, metaphysical realism. In an important sense, this fragmentation reflects the fragmented nature of the post-modern condition (Lyotard).

But to group together all or even most of the discussants in the current debate as "postmodernists", would be an oversimplification. At the most basic level, the term "postmodernist" must be understood as a mere descriptive term which refers to the fact that we have arrived in an era which has superseded modernism. But it can also be regarded in a normative way as referrring to one's opposition to and rejection of the basic tenets of modernism. In the latter, and perhaps more interesting case, the meaning of modernity/modernism needs to be unravelled.

2. (Logical) positivism as the epitome of modernity

Although there might be some disagreement about detail, philosophers of science would probably concur that the following are the main tenets of the modernist image of science:

- The assumption that reason is the main instrument in human (and therefore also scientific) progress.
- The view that objective science implies the acceptance of a value-neutral attitude.
- The view that truth and certainty are the true goals of the scientific endeavour.

In short, within the modernist paradigm, only scientific knowledge is regarded as meaningful, useful and, therefore, legitimate human knowledge.

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These ideals formed part and parcel of the positivist and especially the logical positivist view of science. The main difference between early positivists (Comte, Mill, et al) and the logical positivists, was the latter's emphasis on logic as the best tool to guarantee the rationality of scientific research. Unfortunately, as often happens, this emphasis on logic developed into an obsession. The whole scientific process became to be equated with that which could be translated into or reduced to the rules of logic. In 1945 Carl Hempel could write that "it seems reasonable to require that the criteria of empirical confirmation, besides being objective in character, should contain *no* reference to the specific subject matter of the hypothesis or of the evidence in question; it ought to be possible, one feels, to set up *purely formal criteria* of confirmation in a manner similar to that in which deductive logic provides purely formal criteria for the validity of deductive inference (1945:10)". According to this approach, which McMullin (1979) labelled "logicism", everything *subjective* needs to be ruled out in the discourse of science. Science has to do with the purely formal, neutral and hence, objective.

It is not surprising, that the large scale rejection of the logical positivist view of science in the sixties, would lead to the emergence of views which tended to stress various aspects of the "subjective", whether the historical (Kuhn) or the sociological (Edinburgh school) or even the totally a-logical (Feyerabend's epistemological anarchism) variety.

3. The post-positivist scene

With the ending of the domination of the logical positivist era in the sixties, a proliferation of approaches was to be expected. Some of these approaches were relatively new "inventions"; others were the offspring (and new versions) of paradigms that had been around for a long time. As was indicated above, some can be classified as clearly anti-modernist (or anti-positivist); others as at most post-positivist.

What are the current trends and issues in the methodology and philosophy of the human/social sciences? The list which follows, does not pretend to be exhaustive or even accurate in detail. It is rather an impressionistic overview of some of the issues/debates which are currently (seventies and eighties) attracting the attention of writers in the field.

Paradigmania

An immediate effect of the work of Thomas Kuhn was an interest in describing, demarcating and criticizing paradigms in one's particular discipline. Large numbers of scholars turned their attention to constructing typologies of the current and dominant paradigms in their disciplines. One of the more important consequences of this new "industry" was the re-evaluation of the epistemic status of theories and models in a discipline. The modernist belief in truth, rationality and objectivity and even the ideal of a unified science, gave way to a much more pluralist and in some cases relativist, picture of sociology, psychology, etc.

Rationality and relativism

Both in epistemological and sociological studies of the social sciences, a new interest in issues of rationality and objectivity came to the fore. This is reflected in a number of publications in the seventies on topics such as cross-cultural understanding, the relativism of conceptual frameworks, the various debates on the nature of rationality, and so on. Perhaps the most vociferous exponents of a thorough-going relativism are the scholars of the so-called Edinburgh school in the sociology of knowledge (David Bloor and Barry Barnes).

Discourse fever

The impact and acceptance of Kuhn's paradigm concept, also had the effect that similar constructs, such as research programme (Lakatos), schemata (Wisdom), and especially *discourse* (Foucault), attracted a number of followers. Foucault's various studies on historical discourses (epistemes) and especially his emphasis on the role of power relations, and hence, ideology, in the history of science, generated a series of very interesting debates.

The critical turn

In various debates, and within very diverse frameworks, a new interest in the role of power and ideology, was kindled. Critical theory received a new impetus through the works of Jürgen Habermas in Germany and Brian Fay in England. Hermeneutics also took a critical turn in the work of Paul Ricoeur, while a new school of so-called "Marxist" realists (Bhaskar, Keat, Urry) published a number of stimulating studies in the seventies and eighties.

The revival of realism

A more orthodox response to the demise of logical positivism is to be found in the work of realists such as Hillary Putnam, Richard Boyd, William Newton-Smith and Ernan McMullin. Although they reject some of the major tenets of logical positivism (such as the logicist principle), they are not prepared to reject the belief in reason and objectivity as well.

In general, then, we are currently witnessing an extremely diversified, fragmented and arguably also the most interesting phase in the history of the philosophy of social sciences. We are still in the interregnum with no clear indication of who will be the victor - if there is going to be one!

The papers brought together in this volume, which already constitutes a selection of the papers presented at a conference, reflect this diversity in more than one way. The conference, *Knowledge and Method: A Conference on the Philosophy and Methodology of the Human Sciences*, was held at the Human Sciences Research Council in Pretoria from 25-28 January 1989. Not only do the authors represent a varietyof disciplines in the human sciences, they also subscribe to very different conceptual frameworks and paradigms. Furthermore, the issues which are discussed in these papers, vary from philosophical and meta-theoretical to much more concrete and practical concerns of research practice. The editors believe that this selection provides the reader with a good overview of the current debates in this area.

4. Ten themes

The thirty five papers selected for inclusion in this volume are arranged in ten thematic categories.

1. The first three papers by Hesse, Miller and Mouton, are concerned with broad issues of *metatheory in the social sciences.*

Hesse [1] takes as her point of departure the current debate between realists and relativists in the philosophy of natural science. She proposes a resolution of the debate in which a moderate realism is combined with the acceptance of the epistemic relativity of theories. Hesse then considers the implications of such a position for the human sciences.

In a critique of Bhaskar, Miller [2] points out inherent ambiguities and contradictions in Bhaskar's transformational model of social activity. According to Miller, Bhaskar does not escape the errors of voluntarism and reification which he has set out to refute.

Johann Mouton [3] discusses issues related to the relationship between values and objectivity in the social science. In the first section of his paper, he briefly reviews the history of the traditional notion of objectivity. In the second section he argues that an interest in power is an intrinsic feature of all social sciences research, while the third section is devoted to a discussion of the dominant traditions in current philosophy of social science with in each case an emphasis on the specific metaphor of power. In the final section he asks the question whether it is possible to defend the ideal of objective social science, while also maintaining that social science is intrinsically value-laden and political.

2. *Recent trends in specific disciplines:* Psychology, sociology, education and anthropology are reviewed in four papers.

Du Preez [4] analysis two problems: The first is the distortion of psychological knowledge by practices in publication; the second is the relation between macro- and micro-psychology. With regard to the second problem, he maintains that this relation can be understood in two ways: [i] Macro-psychology is to micro-psychology what Newtonian mechanics is to quantum mechanics. [ii] The relation is that of "stance". The proper stances of the psychologist are intentional [folk psychology] and design [theoretical psychology].

Joubert [5] in *Towards a human sociology*, writes on the similarities and differences between common social sense and scientific social knowledge. He distinguishes a number of "contrapositions" within social knowledge which allows for more or less human characteristics inherent to social reality. The inability to consistently appreciate humanness, Joubert argues, is the price exacted by sociology's commitment to the scientific method in understanding social reality.

Morrow [6] reviews educational methodology and concludes that there are [in South Africa] no trends to be discerned. He subsequently discusses a recent investigation into research priorities in education and reveals considerable confusion. Morrow also claims that because of an inadequate theory of meaning, much educational research misses its mark.

According to Thornton [7], the most significant trend in cultural anthropology today is the formulation of a diffuse but powerful programme for a post-modern anthropology. While neither "modern" nor "post-modern" approaches can be precisely defined, they partly define each other by their opposition. This paper sketches some of the salient features of each, and examines variant approaches to the question of holism and ideology within contemporary anthropology.

3. The five papers that follow by Louw, Peeters, Sapire, Van Niekerk and Vorster all largely deal with *issues of interpretation and rationality.*

Louw [8] argues that the development of philosophical hermeneutics has led to the overcoming of traditional epistemology. This stimulated a renewed interest in the question of method. This issue is illustrated in a discussion of aspects of Dilthey's hermeneutics.

Peeters [9] questions two commonly held presuppositions: [i] That science should have a practical application and, [ii] that interpretation in general is subjective in the pejorative sense of the word. He argues that the rejection of interpretation is based upon a false conception of its nature.

In a critique of Jarvie, Sapire [10] identifies a number of inconsistencies in the former's notion of rationality. His discussion centers on two issues: the nature of rationality and the question of the psychological unity of mankind.

The paper by Van Niekerk [11] is an investigation of the applicability of Brown's model of rationality to the human sciences. The author comes to the conclusion that judgement is an essential part of a model of rationality in the human sciences, but that Brown's model does not address the issues of collective judgements [i.e. judgements informed by tradition] and social relativism adequately. The paper is concluded by suggestions about how this deficiency can be met.

Vorster [12] explains differences in current interpretations of the Jewishness of Jesus by analising the presuppositions on which these studies are based. Presuppositions which are related to philosophy, theology, sociology of knowledge, text theory and language, and which play a role in these studies, are discussed.

4. Studies in the *morality and ethics in the practice of social research* are presented by three contributions.

Based on a study of censorship in South Africa, Merrett [13] makes a distinction between "traditional" and "radical" forms of censorship. Whereas traditional control of publications can be circumvented, more recent radical censorship [such as the state of emergency] has had devastating effects on research and calls for a response by the research community.

The objective of the paper by Preston-Whyte [14] is not only to raise the general issue of research ethics in the social sciences in this country, and at this time, but, more specifically, to consider the appropriateness of the formulation of guidelines for professional conduct, either within particular disciplines, or for the social sciences as a whole.

Rauche [15] argues that, since scientific methods are "man-made" no scientific theory can be conclusive; they remain problematic and controversial even in their relationship with each other. According to Rauche this "critical relationship" reflects man's basic experience of life, and points to an interplay of theory and practice.

5. Three papers in the field of *cultural and media studies* are grouped together.

Fourie [16] explores two paradigms in research on mass communication, i.e. the administrative (positivist) and the critical paradigms. He shows that in the former, mass media are perceived as neutral tools serving a wide variety of objectives; in the latter, the emphasis is on the interaction between media institutes and the socio-political environment.

The problem and challenges of analyzing the "alternative media" in contemporary South Africa are addressed by Louw [17]. He argues that the historical materialist approach is valuable in studying this phenomenon, one reason being that it has had an influence on alternative media practice.

The study by Muller and Tomaselli [18] traces the lineage of various forms of cultural studies in South Africa with special reference to Afrikaner conservative cultural theory, the liberal humanism of English speakers, Marxist radical anti-humanism and democratic radical humanism. These intellectual movements are examined in relation to the history of class and group conflict in South Africa.

6. Three papers are concerned with *issues of discourse and power.*

In a more philosophical analysis of Foucault's project, Frigerio [19] shows that his aims are to demonstrate how the human subject is the result of the working of power/knowledge technologies which operate independently of human control. This "autonomy" thesis has been regarded as a form of nihilism which "explains away all meaningful interpretations as a form of illusion". Frigerio argues that Foucault's account of the working of power/knowledge technologies does not imply such a nihilistic conclusion and that in fact it offers us guidelines on how to make meaningful choices to oppose the objectifying force of power/knowledge apparatuses.

Kottler [20] analysed male-female conversations to identify possible sex-differentiated speech. She concludes that the method of conversation analysis is not incompatible with more comprehensive discourse analysis.

According to Levett [21], current research on the effect of child sexual abuse exemplifies a kind of empiricism which precedes and perpetuates particular forms of knowledge. This is contrary to the methodology of discourse analysis which enables one better to understand the nature and effects of everyday practices and to recognize gaps and contradictions in dominant discourses and thus to modify them.

7. We have put five papers together that all deal with *the phenomenological and other qualitative views* of social science.

Edwards [22] argues that the case study methodology which has been described in detail in a number of recent publications is the implicit basis of the advancement of knowledge in psychotherapy. However, its status as research has been inappropriately ignored because of the preference for quantitative methodologies and the use of statistical inference. The basic principles of case study methodology are outlined, and the core rôle it plays in clinical research is discussed.

In a more philosophical study, Fouche [23] criticizes Husserl's distinction between philosophers' and human scientists' application of the phenomenological reduction. She suggests that Merleau-Ponty's view that the epoche as a tentativeness about all assumptions is likely to lead to sounder and more sensitive psychological practice. It is the Merleau-Pontyean version which lends itself to effective ideology critique. Fouche maintains, in conclusion, that there are significant correspondences between the phenomenological reduction and the dialectic in that both methods can accommodate tensions and ambivalences.

Hoek [24] illustrates the application of phenomenology in a study of self-awareness as it develops in relation to others.

Kruger [25] claims in his paper that the conception of truth subscribed to by psychologists and psychotherapists, is still dominated by Cartesian rationalism. According to this position truth is embodied in clear and distinct ideas formulated into a statement which corresponds to the actual objects. The aim of this paper is to show that this conception is unsuitable for psychology as a human science, that coming-to-know in psychotherapy involves more than pure cognition, and that "truth as disclosure" (*Entbergung*) may be more suitable.

The problem of the incommensurability of paradigms in psychology is the topic of O'Dowd's [26] paper. He argues that there are substantial, albeit partial, convergences between psychiatry and psychoanalysis, and between psychoanalysis and the critical positions. However, to work with these, and to work through them to a multi-dimensional model requires tolerating both logical and methodological ambiguities, and referring again and again to experience for clarification.

8. An eighth theme concerns *issues of conceptualization in the study of government and law*.

The paper by Atkinson [27] presents various ways of approaching the study of government departments. It demonstrates the intersection between two different distinctions: [a] that between intra- and extra-institutional approaches, and [b] the difference between relatively concrete ontologies and more abstract or imaginative conceptual schemes. Two possible constellations are presented: one based on notions of conflict, power, ideology and interest, and another based on consensus, social order, commitment and legitimacy. Finally, the phenomenon of information processes within a bureaucracy is analysed as an example, to show the various choices a researcher can make when choosing a theoretical perspective.

Sociologism, Hund [28] maintains, is the view that all types of knowledge, including knowledge of justice and rights, are relativistic sociological constructions and not timeless Platonic forms. In this paper he outlines a sceptical ideology of justice and rights that has a following in South Africa, and likens this view to sociologism. Without attempting to "refute" sociologism, connections between it and the South African ideology are explored and several objections to this relativistic theory are raised.

According to Van Reenen [29] comparative law as a legal discipline can be said to have reached an impasse. It is his opinion that this state of affairs is due to the non-recognition of the methodic meaning and relevance of a fundamental, ontic concept of law. In this contribution, he establishes the critical importance of a valid transcendental theoretical legal concept as basis for a fertile comparison of positive legal figures (institutions) in various legal systems.

9. Issues of *understanding and explanation peculiar to anthropology* constitute the theme of the next two papers.

According to Erasmus [30] the understanding of societies other than one's own presupposes an acceptance of the principle of the uniformity of mankind. The author chooses for this position after having reviewed classic evolutionism structuralism, phenomenology and cultural relativism.

For some anthropologists the ontological status of social units is self-evident and therefore unproblematical. For others, the interaction between social units and the variation in objective and subjective group definitions, have become important problems. Van der Waal [31] criticizes both social anthropologists and "volkekundiges" in South Africa for their uncritical use of "ethnic groups" as basic units of study.

10. Papers by Barnard, Hesse, Muller and Van Straaten address *paradigmatic shifts in the disciplines of archaeology, geography and planning theory.*

Barnard [32] reviews the historical development of geography, concentrating on various paradigms such as environmental determinism, possibilism, positivism, critical rationalism, phenomenology and especially, radical geography and realism.

The discipline of town and regional planning has, since the early 1970's, says Muller [33], claimed a proprietal right to a particular field of theory, viz procedural planning theory. Its claim to sucha privileged theoretical status resides largely in its attachment to the philosophy and principles of rationality. This attachment has precipitated a paradigmatic impasse in the discipline. The impasse results mainly from the current radical critique of procedural theory, which contends that the rational procedural model is severed from reality; that it separates itself from the socio-historical and political contexts of the real world, and is consequently without functional utility.

Archaeology has the special feature that its theories are necessarily about past events that cannot be observed any more. The methodological problem that Hesse [34] discusses, is: how to infer from material remains and their natural environments to the socio-cultural features of the peoples who left them behind.

In the final paper, Van Straaten [35] starts with a sketch of the methodology proposed by Popper for natural science; and then specifies the model presented by Imre Lakatos for assessing when one research programme or theory should replace a rival. This is followed by a sketch of the problem of method in archaeology. He suggests a way to unify the methodological diversity by using the Lakatos-Popper methodologies. The model is then applied to two debates (which may be regarded as either current or recent!):palaeontologists/physical anthropologists versus bio-scientists; and sociobiologists versus social theorists.

5. Acknowledgments

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A special word of appreciation is due to Mrs Susan Smith who took care of the typesetting of the manuscript.

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Metatheory in the social sciences

1

Beyond relativism in the natural and social sciences

Mary Hesse

Realism in natural science

In current philosophy of natural science the debate between realists and relativists has become endemic and inconclusive. The fortunes of the contestants swing to and fro with fashion, and with their own philosophical interests, but so much about the character of natural science is agreed on both sides that the fruits of victory hardly seem to justify the passion or the paper still expended. In the human sciences on the other hand, the battle is one of life or death, for it concerns nothing less than the *possibility* of any kind of knowledge in the human sphere, and if knowledge is possible, its nature and justification are what is at issue. In this paper I shall present what seems to me an acceptable resolution of the problems in the natural sciences, involving a moderate realism combined with an epistemic relativity of theories.[1] Then I shall consider what implications this has for the philosophy of human science, and argue for the rejection of concepts of knowing, truth and explanation as traditionally based on the natural and human science in terms of the social concepts of mental modelling and cognitive systems.

There is a form of realism about science that says simply that things exist that are independent of all human knowledge and belief about them. This is an epistemologically empty view unless more is said about the *kinds* of things that exist and how we know them. It is indeed difficult to imagine any culture in which there is no assumption of the existence of common-sense objects (sticks, stones, people ...), however they may be described and in whatever world-model they are embedded. But realism about scientific objects is a different matter. Given the history of cultures it is not at all obvious that they should be given privilege over other kinds of more problematic entities sometimes postulated to exist, such as gods, souls, social norms and moral values. To do so is to assume privilege for the claims to knowledge arising out of the institution of

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science, and this already begs questions about the realism of scientific theories, and demands scrutiny of the means whereby we are said to attain such knowledge.

It is of course impossible to discriminate absolutely between the directly observable common-sense world, and scientific objects which are indirectly observable only through their effects. Consider Jupiter's moons. We now accept these as almost common-sense objects that were waiting to be discovered and described. They are now part of irreversible positive knowledge, but they were at one time regarded as highly theoretical and speculative. They were visible only in the newly constructed telescope, and the observations were unreliable and open to a variety of interpretations. The "moons" might have been optical artefacts or accidental conglomerations of matter somewhere between lens and planet. They are now accepted as common-sense objects for many reasons involving their relations with other common-sense objects and behaviours. They persist in time and are reidentifiable. They move regularly around Jupiter according to the same laws that describe the motion of our our moon. Young astronomers can be taught to recognize them just as they learned to recognize Westminster Abbey or the Statue of Liberty, with the aid of both observation and verbal description, but without a lot of theoretical talk. But when Jupiter's moons were subjects of scientific controversy, theoretical talk was necessary, to spell out the inferences justifying the account of certain apparent objects as *moons* related to the *planet Jupiter*, all of which terms were and still are "theory-laden".

This example shows that the fact that terms referring to more or less directly observable objects are theory-laden does not entail that such objects "do not exist", nor that they cannot be described in sentences accepted as true. And the same can be said of any other scientific objects postulated in theories. Consider the example of "electrons". We know a great deal about small objects that have a certain measurable charge and mass, instances of which are identifiable and distinguishable from other types of micro-particles in a great variety of experiments. "Electrons exist". Everything we know about electrons has become part of the deposit of positive science, just as have Jupiter's moons. But the fundamental theories of micro-physics are controversial and at present highly volatile. Nothing can rule out the possibility of a "revolution" in physical theory that will reinterpret many identifications and laws about electrons, in a way similar to what happened, for example, to phlogiston. There are two aspects to such revolutions. First they *leave alone* many local and particular truths, where we may as well say that we are referring to "electrons", even though in a new theory electrons may not have all the apparently essential characteristics we now postulate of them. Hydrogen goes up in flames even if called "phlogiston" in Priestley's experiments; electrons by any other name activate cathode ray tubes and geiger counters. But in a different sense the new theory may show that "electrons do not exist", because our present theory about what they are and how they behave is found to be in detail and in general false. In just the same way we now say "phlogiston does not exist" because Priestley's theory is in detail and in general false. False, that is, of the truths that have now entered common-sense realism.

This argument is enough to justify a distinction between common-sense and scientific realism, even though the boundary between what are taken as accepted common-sense objects and theoretically dubious scientific objects shifts with history and knowledge. But several important loose ends remain. The first concerns the question of particularity and generality. Nothing that has been said so far about common-sense object implies that we know more than *local* and *particular* things about them. We do of course utter generalities and predictions about them: we say sticks will burn and stones will not, that Jupiter exerts a gravitational force on his moons, that water consists of hydrogen and oxygen atoms. But common-sense is wise enough to leave the meaning of such assertions vague when extrapolated to imaginary and farfetched conditions where we are not justified in supposing local regularities to persist. What counts as "burning" under conditions of nuclear explosion? What might happen to Jupiter's moons in the future evolution of the sun? What is meant by "atoms" in the first microseconds after the big bang? These are only some of the unfamiliar conditions we can now foresee; there are indefinitely many others we cannot foresee, and in all these cases we do not now have satisfactory theories that enable us to answer these questions. Precise semantics and precise laws belong only to specific theories. Our specific theories are not complete, and it is doubtful whether they ever could be, however much theoretical "progress" there may be in the future.

The local and particular success of science does not require there to be true *general* laws and theories, even about common-sense objects, or at least it does not require us to know them. We are always in local and particular situations, however far into past or future or into the micro-scale our theories claim to penetrate. Moreover, our application of general terms to common-sense objects, as well as to scientific objects, is always subject to ambiguity of meaning when extrapolated to distant conditions. We are trapped in some natural language or other when we talk about the real world, and because we never know whether our theories are strictly true, we do not even know how to express the properties of objects under highly extrapolated conditions. Local meanings are contextual, and work without having to capture natural kinds or essence, and without natural kinds we cannot express general laws.

So far we have noted two features of a moderate realism for natural science that are going to be relevant to the social sciences, that is, the *theory-ladenness* of observation, and the absence of the need to postulate strictly universalizable *scientific laws*. A third feature restores a sense of objectivity to science. This is what may be called the *pragmatic criterion*, namely the progressive character of scientific success in ordering the (always "local") natural environment to permit prediction and control. This seems to me to be the crucial demarcating criterion and the overriding cognitive value of natural science as this has been institutionalized since the 17th century. It is an instrumental criterion, but it does not prevent us interpreting theories as descriptive of real structures of the world (regularities, measures, symmetries), though not as descriptive of ontology - the ultimate "what-is" of the natural world. The impossibility of knowing the essence of nature in this sense is what makes this realism "moderate".

Are natural and human sciences irreducibly different?

In the second part of this paper I want to derive some consequences for realism and relativism in the human sciences. Those who oppose positivist-type methodology in the human sciences have pointed to several kinds of differences, and divide them into two groups according to how their characterization is affected by the moderate realism for natural science argued for in the first part.

REDUCIBLE DIFFERENCES

	Positivist interpretation of natural sciences	Hermeneutic interpretation of human science
R.1	Data direct, theory-independent	Data interpreted
R.2	Objective laws and concepts	Correlations and ideal types
R.3	Language exact, univocal	Language equivocal, contextual
R.4	Concerned with closed experimental systems	Concerned with open systems

There are four commonly suggested discriminations which on examination turn out to be only more extreme cases of the moderate realist construal of natural science. I shall call these *reducible differences*:

R.1 It is suggested that in natural science observational data are directly measured, objective and theory-independent, while in human science data are indirectly derived, e.g. from interviews, documents, artifacts, etc., and do not become data until they have already been interpreted and processed by the observer's interest. But we have seen that the language of observational description is always theory-laden with the taken-for-granted categories of background knowledge and cultural assumptions, and that this leads to epistemic relativity in theories in natural as well as human science. In neither case can we expect final definitive conceptual systems or theories, and there is no convergence towards true theoretical description of the real. It does not, however, follow from this that there are no objective constraints upon better and worse theories, and no progress of natural science towards pragmatic goals.

R.2 It is suggested that laws and theoretical entities discovered in natural science are external and objective, while in human science there are no laws properly speaking, but only empirical correlations of tendencies which are dependent on categories of description, and there are no objective theoretical objects but only ideal types constituted by observer-interests (Weber's "value relevance"). But I have argued that there is no need to postulate universal laws or causal necessity even in natural science, since all that can be confirmed are theoretically constituted regularities, applicable in essentially finite local domains of space and time. For example, Newton's theoretical objects and relations such as force and mass are partly constituted by Newton's laws, and these laws are "true" only in limited domains. Moreover theoretical models in natural science cannot attain to ideally true descriptions, and function very much like ideal types in social science, in that they express the background concepts and interests presupposed by the community's language and cognitive systems.

R.3 It is suggested that language in natural science is exact, formalizable, literal and univocal, whereas language in human science is essentially interpretive, equivocal, and adapted to particular contexts. This suggestion is sufficiently refuted by the theory- and concept-ladenness of all natural language, and the earlier remarks about the use of laws and models with limited application to particulars, whose meanings are therefore context-bound.

R.4 Perhaps the most interesting suggestion (made principally and in detail by Roy Bhaskar[2]) is that the subject matter of natural science lends itself to the experimental construction of *closed systems*, within which causal laws can be identified as real structures and can form the basis of realistic explanations. Bhaskar argues that even in natural science closed systems do not occur naturally, and that when theories are applied in *open systems*, laws become tendencies, and theories are not conclusively testable by detailed predictions. In his view this brings applied, but not pure, natural science, into line with human science, where it is generally not possible to set up closed experimental systems in which to discover and test systematic laws and theories. Laws in human science are also represented by tendencies in essentially open systems, and hence are not predictive, at least not over domains removed in space and time from their origins. So in terms of Bhaskar's distinction applied natural science is like human science, but pure science is not. Is his distinction sustainable, and is it consistent with the moderate realism about pure science I have outlined?

Since Bhaskar is one of the few philosophers to construct a realist theory of both natural and human science, while taking full account of their different types of subject matter, this is the place to say a few words about his arguments. With regard to the natural sciences, his realism is not inconsistent with what I have adopted above. Bhaskar's is an *ontological* realism combined with the recognition of epistemic relativity. That is to say, he believes in a real causal structure of the world, which both provides the empirical constraints upon science, and envisages that in closed system systematic explanation and prediction are possible. But he acknowledges that our theoretical *descriptions* are contingent upon changing linguistic categories and cultural interests, and does not argue for any convergence to ideally true theory within actual historically evolving science.

I have characterized such ontological realism as empty if it tries to speak of the unspeakable content of real causal structure, but like Bhaskar I have tacitly adopted a sort of ontological realism in recognizing experimental tests and successful prediction as ultimate constraints on natural science theories, in what I called the *pragmatic criterion*. Bhaskar, however, is not satisfied with pragmatic success as the goal of science, and wishes to give more prominence to its *explanatory* function as the basis of his realism. This is where his account of human science as purely explanatory, though not predictive, parts company with mine, because I do not admit a purely explanatory function even in natural science.

Let us clarify the issue by considering Bhaskar's account of explanatory laws in closed systems, which become tendencies in open systems both in the natural and human worlds. I have argued that it is unnecessary to interpret scientific laws as universally quantifiable sentences, much less to impute any kind of necessity to them. They are the product of empirical correlations reconstructed coherently within theories, and have no stronger status than Bhaskar's "tendencies". Tendencies do not have to be grounded in causal structure that can actually be discovered *and expressed* even in closed systems. In any case the distinction between open and closed systems cannot be pushed too far even in pure science, because most closed experimental systems are highly artificial objects and not universalizable in the way causal laws are supposed to be. On the other hand there are approximations to closed systems which do occur naturally, for example the solar system, and here wide-ranging predictive tests are possible. So the pure/applied distinction is not coterminus with the closed/open distinction. Both closed and open systems may support lawlike statements, since all that is required is sufficient predictivity in finite space-time domains, domains that may be comparatively large in the case of physics or cosmology, but comparatively small in human psychology or human society and history. To require more than finite applicability and predictivity is to carry over too much of the standard theory of deductive explanation which Bhasker is in other respects concerned to refute.

The notion of a "wholly explanatory" theory collapses along with the idea of an explicitly describable real causal structure. In natural science, if a theory is not in any sense predictive, there is no constraint upon the quality of the explanations it provides. Explanations are necessarily couched in theoretical language, and this is epistemically relative. But if it is impossible to retain the notion of pure explanation within a natural science realism, great difficulties follow for any realistic theory of human science where testable predictions are generally excluded. We have to face the possibility that in the absence of pragmatic tests, there is no knowledge (in the standard sense) in the human sciences at all.

This brings us to four more characteristics of human sciences, which most philosophers, including Bhaskar, have acknowledged to make them irreducibly different from the natural sciences.

IRREDUCIBLE DIFFERENCES

	Natural science	Human Science
D.1	Unified comprehensive, progressive theories, hence predictive	Almost none such, hence mostly non-preeictive
D.2	Overiding pragmatic criterion	No single overriding criterion
D.3	Only marginally concerned with interests and values	Concerned essentially with interests and values
D.4	Non-self referential	Self referential

D.1 There are no systematic, unified, widely applicable and progressive theories in the human sciences comparable with those in the natural sciences. This is a matter of historical fact, and is explicable in terms of two assumptions:

[i] That to be comparable with the natural sciences, human sciences would have to adopt the pragmatic criterion as overriding goal. I have argued that this criterion is definitional of what we mean by natural science.

[ii] The pragmatic criterion is not generally applicable in the open systems that constitute human-science subject matter, hence there is no basis for the progressive accumulation of good theories such as exist in natural science. If such background theories are lacking, there are no criteria of coherence with established theory which can be used to judge the goodness of explanations in human science, thus further undermining Bhaskar's contention that these sciences are purely explanatory.

D.2 I have said that the pragmatic criterion is the overriding *value* in the natural sciences. Historically speaking this accounts for their progressive character in satisfying their goals irrespective of temporary attachments to other types of value (e.g. coherence with ideological, political and social interests). But the pragmatic criterion may not even be desirable as an overriding value or goal in the human sciences. We therefore have a situation in which other types of value are endemic to human science, and it remains to articulate for them types of goals other than predictive success.

D.3 Values, interest, norms and meanings enter into human sciences also in another sense. Their categories of interpretation carry meanings and interests already present in the frameworks of the investigators, and those found and expressed in social life by the actors investigated. There is by definition no parallel to this in the natural sciences. It has led to the hermeneutic account of human-science method, in which understanding of the stranger's speech and beliefs has sometimes been made the overriding criterion in sciences such as history and anthropology. More generally, attention has been directed to problems of translation and interpretation between cultures which must form part of any investigation of society and history. Moreover, the human sciences may claim, in the light of their ideological interpretations of society, to bring criticism to bear upon particular features of particular societies. Hence the goals of human science are sometimes taken to include the critique of ideology and the unmasking of and emancipation from the false consciousness of the actors themselves. In other words, the interpreter's values are sometimes held to have priority over actors' reports about their own societies.

D.4 Human sciences are self-referential, in the sense that they themselves form part of their own subject matter: the study of sociology is a social phenomenon, the origins of scientific history in the 19th century is an historical phenomenon, etc. This is without parallel in natural science unless we allow ourselves to contemplate distant or perhaps illusory scenarios such as a successful sociobiology of human cognitive systems including science itself.

Cognitive relativism and cognitive systems

All these differences between human and natural science tend towards cognitive relativism. Adoption of non-pragmatic values and goals, interpretations of meaning, and critique of ideology, all face the question "Upon what basis of value, interpretation and critique do they themselves stand, and with what justification?" If there is to be any realistic interpretation of the human sciences it must rest upon a moral or ideological realism - that is, we must be prepared to find truth in some value-system, and that truth will not itself be scientifically justifiable. Notice that the natural sciences themselves begin to appear more relativistic from the point of view here described. They are the product of a historically contingent social "decision" to adopt the pragmatic criterion as the overriding value and goal of knowledge. In this sense a general historical relativism is inescapable, once the rationalist realism of a priori knowledge is abandoned. But once we situate ourselves within the historical process, this point can be seen to miss the real impact and threat of relativism. For our society there is consensus about the *pragmatic* value, which is almost regarded as a self-evident criterion of objective knowledge, but there is no consensus or even clarity about other appropriate goals of knowledge.

This characterization of relativism, however, still owes too much to the presuppositions of traditional realism. To go beyond the dialectic of realism and relativism in modern philosophy, nothing short of a rejection of the presuppositions of both is required. In the list of similarities and differences between natural and human science I have drawn up, the human sciences have been characterized as if they "fall short" of the criteria of natural science in respect of the absence of systematic theory and satisfaction of the pragmatic criterion. But adoption of the pragmatic criterion is just that historical episode which *defined* for modern philosophy the standards of truth, rationality and knowledge: roughly as "correspondence" of theory with the real world.

A correspondence theory of truth is also of course found in Aristotle, long before systematic empirical testing of theories was developed. But what Aristotle means by it is very different and much more liberal in its ontology. When he defines truth in terms of the aphorism "To speak truly is to say of what is that it is, and of what is not that it is not", no tests of this correspondence relation are immediately proposed. In particular it is not assumed that "what is" is exhausted by the experimentally testable in the sense of later science. "What is" may include gods, spirits and Platonic forms as well as sticks and stones. Aristotle's own criteria for "speaking truly" were philosophical, teleological and intuitive as well as observational, but these were just the criteria explicitly rejected in the "new experimental philosophy" of the 17th century, and we are still uneasy with them.

A different approach to the generalization of notions of truth and knowledge and their justification would be to follow a route much more accessible to our culture, namely the idea of the evolutionary development of cognitive systems. Evolutionary epistemology has been one of the ingredients in current epistemic relativism, but it has within it the potential to resolve some of relativism's more objectionable features. It was Quine who initiated a "naturalized epistemology" by rejecting the notion of fixed a priori truths to which logic and scientific conceptual frameworks must conform. His epistemology describes how we get around in our natural environment by inductive methods, which may themselves be understood as extensions of animal learning. The previous a priori elements of the rationalists become contingent universals of human biology and psychology, whose success is guaranteed by the various mechanisms of selection and survival.

One problem with this naturaiized epistemology is that science, on the face of it, does not look like the natural extension of animal learning mechanisms, nor is it obvious that its theoretical aspects have much to do with the biological survival of the species, indeed at present rather the reverse. But the most important objection is that naturalized epistemology remains incurably *individualistic*. Hints towards cultural as well as biological evolution are not sufficient to take account of the social context of knowledge. Epistemology needs not only to be naturalized but also *socialized*.

We can get help here from several human sciences, ranging from artificial intelligence to social anthropology. These have developed a vocabulary of *cognitive systems*, that is, individual and social systems of belief, which enable us to speak in the first place of what in a given culture is *held to be* valid knowledge. This approach emancipates the study of the history and anthropology of ideas from models of rationality based on western logic and science as paradigmatic norms of thought. Social anthropologists have most obviously been forced to consider modes of thought very different from western rationality, and to take them seriously as cognitive systems. This has useful implications for the search for more general modes of rationality which may mitigate the threat of relativism.

What are the marks of a socially recognized cognitive system? What, on this view *is* the subject matter of a generalized theory of knowledge? The definition suggested by the western philosophical tradition is that a cognitive system is that which commands the belief of most, or a significant number of, members of a society, together with the institutionalized structures, performances and actions associated with that belief. Belief is usually construed here as an individual state of mind, and is related to a theory of rational action, according to which complexes of human action, goals and beliefs may be combined with facts to give explanations of action. Thus an action is rationally explicable if it can be shown that it would be an effective and efficient means of attempting to reach an assumed goal, given the factual circumstances, and assuming the truth of the beliefs of the agent.

This theory can take account of beliefs that are not scientifically grounded and goals that do not appear to us to be rational. It can, for example, explain why medieval landlords gave valuable land to the Church in exchange for prayers for the souls of their ancestors. But still the theory remains too close to western scientific categories to be entirely adequate. Where belief and action in an alien society is not "scientific" in our sense, there may be no check on what the beliefs actually are, other than circular checks within the whole complex of ritual acts and affirmation, and even these are not directly describable and have to be interpreted. Is a ritual ceremony an invocation of the gods, or is it the community's way of bringing psychological pressure upon individuals to conform, or is it a method of curing psychological ills? Even the assumption within the rational action model that there will be an intention to use "effective and efficient means" to a desired end is a modern technological concept that may not be applicable. For example, as Weber recognized, some actions are "for their own sake"' but no less "rational" for that.

A better approach, which can make room for both the parallels and differences between scientific and "ritual" belief systems, draws on the cognitive sciences themselves.[3] Studies of mental modelling of the world are clearly relevant to the ways in which both science and other belief systems enable humans and animals to survive and realize their purposes. Such mental modelling is not mere copying. It is already evident from work on elementary perception that no one-to-one correspondence theory of truth provides an adequate theory of modelling even of the natural environment. "Mental modelling" must be taken to include structures having various analogical and morphic relations to the world, which may require elaborate transformations and mediations through action before the "world as experienced" can be reconstructed. Moreover, models may be hypotheses or "stories" about the world, which issue in testable action. The success of a model may be tested by comparisons of the expectations induced by it with further experiences, or by longer term biological selection of overall purposes both manifest and latent.

So far little is known about how mental schemas which are social rather than individual (Durkheim's "collective representations" or Leach's "symbolic statements") work as cognitive systems. Functionalism, structuralism, and sociobiology are all theories of their operation, and none has been found generally adequate. Even if there were a more satisfactory theory, however, the question would still remain as to how far such a notion of the cognitive can escape the perils of relativism. At first sight it seems merely to institutionalize relativism by defining knowledge as bound to the belief system of a given society. It is certainly not, however, a relativism of "anything goes", because its social basis ensures that there are powerful social and natural constraints upon belief and its manifestations. There are certain things that in a given society it is literally impossible rationally to believe in any sense that can issue in action. For example, in most modern societies we cannot actively believe that every human death is due to some malevolent and socially accountable human agency, since the social substructure for actualizing such a belief is not present. But this only places the cognitive systems of a society within limits, and in modern societies it does not even mean that there is a unique system which dictates detailed beliefs to every member. Different cognitive systems define different cognitive sub-groups - ideologies, political parties, religions.

Sub-groups may be in conflict with each other. As long as conflict remains argument and not violence, the arguments depend on a variety of criteria of the social good and the factually possible, which both sides may be persuaded to accept in common. This is how the normal process of democratic politics proceeds, and how rational horizons may be "fused" in Gadamer's phrase, in the hermeneutic attempt to communicate with alien societies. An overriding criterion which is generally acceptable in modern society is that of Kant's prescription of universalizability. The "conversation of mankind" carried on between distinct cognitive groups should always strive to take in as many horizons as possible, not only those that are contiguous in time and space, but those belonging to our own and others' historical traditions, and as far as possible those spacially and culturally distant. Historical facts themselves have now determined that for better or worse the conversation is worldwide and situated in an informed historical consciousness as never before.

In the end, however, the sting of relativism remains. It lies in the denial that in the last resort there is any Archimedean point on which to stand to judge between the ultimate values of conflicting cognitive systems. I do not have any panacea to resolve this ultimate problem. But there are one or two lessons to be learned from the particular example of the cognitive system which is natural science, and the debates about realism and relativism that have surrounded it, and with these points I summarize and conclude my argument.

Firstly, the moderate realist analysis of natural science shows how it is possible to distinguish existence from the tests for existence, ontology from epistemology. The remarkable, and ultimately contingent, fact that there is a wealth of relatively stable regularities in the world, and that we can come to know more and more about these, is both a presupposition and a confirmed discovery of natural science, and it is a fact about existence. This fact satisfies the requirements of all realisms, that things should exist independently of human observation or knowledge of them. In the case of these particular existents, we have learned about them by exploiting a systematic experimental method of trial and error and repeated tests, and by adopting the pragmatic success of this method as the overriding goal of scientific knowledge.

Secondly, a moderate realism of natural science is perfectly compatible with a relativist construal of laws and theories as locally applicable with respect to truth, and context- and theory-bound with respect to meaning. It follows that natural science cannot logically refute the content of other cognitive systems where this content escapes the pragmatic criterion.

Thirdly, there are cognitive systems including our human sciences, which are constrained by criteria other than pragmatic success in the natural world. These criteria are generally concerned with intra-social goals rather than goals which primarily relate society to nature. They include the norms and mores which contribute to the possibility of social life and the regulation and resolution of conflict. They vary widely from society to society. The human sciences in particular involve decisions about underlying values and ideology which are just as rationally ungrounded as the Enlightenment has taken all non-scientific cognitive systems to be.

Fourthly, there are certain natural biological, social, and power constraints upon all viable cognitive systems, which mean that in any given society, including our own, there are some things we *cannot* believe or even think. So ultimate relativism, for these contingent reasons, does not leave us as bereft of guide posts as it seems to, from within a purely intellectual perspective. Social determinism is stronger than we think, though not complete. It is often our duty to resist it, but in doing so there is no a-social or a-historical rationality from which we historical beings are able to start. In particular, *philosophy* cannot provide one, any more than it does for conflicts in the natural sciences. Cognitive systems are relatively autonomous phenomena: social, moral, religious, but not philosophical.

Notes

1. This view is developed in more detail in my "Science beyond realism and relativism", in *Cognitive relativism and social science* ed. D. Raven, L. van Vucht Tijssen and J. de Wolf, 1988, University of Utrecht, pp. 129-143.

2. Roy Bhaskar, 1975, *A realist theory of science*, Leeds Books Limited and *The possibility of naturalism*, 1979, Harvester, Brighton, Sussex.

3. Cf. M.A. Arbib and M.B. Hesse, 1987, *The construction of reality*, Cambridge University Press.

2

A critique of Bhaskar's transformational model of social activity*

Ronald Miller

Bhaskar's central argument is that society or social structures are real in the sense that they generate human action. His "transformational model of social activity" is an attempt to relate society and people without committing the errors of voluntarism or reification that he argues are inherent in Weberian and Durkheimian approaches, respectively. Moreover, his model attempts to correct "in a fundamental way" dialectical approaches that compound both the above errors. He argues that people and society are not related dialectically. Rather they refer to radically different kinds of thing" (1979: 42). Although the model has been extended (Bhaskar, 1986: 155), it remains at the core of his attempt to provide a realist account of the human sciences.

The fundamental assumptions upon which Bhaskar's conceptions of society and people are predicated are as follows:

> Men do not create society. For it always pre-exists them. Rather it is an ensemble of structures, practices and conventions that individuals reproduce or transform. But which would not exist unless they did so. Society does not exist independently of conscious human activity (the error of reification). But it is not the product of the latter (the error of voluntarism) (1979: 45-46).

The model that Bhaskar proposes is represented diagrammatically below and he explains as follows:

> Society, then, provides the necessary conditions for intentional human action, and intentional human action is a necessary condition for it.

* The original title of this paper has been changed at the request of the editors.

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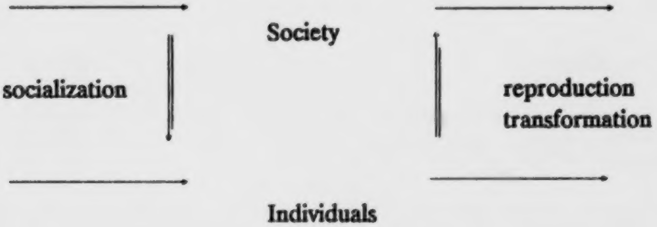
> Society is only present in human action, but human action always expresses and is only present in human action, but human action always expresses and utilizes some or other social form. Neither can, however, be identified with, reduced to, explained in terms of or reconstructed from the other. There is an ontological hiatus between society and people, as well as a mode of connection (viz. transformation) that the other models typically ignore (1979: 46)



The Transformational Model of the Society/Person Connection (1979: 46)

Although the basic premise, that people do not create society because it always pre-exists them, derives from Durkheim, Bhaskar's conception of social structures are very different to Durkheim's formulation of social facts. The social structures that Bhaskar either derives from his model, or that he uses to construct his model, have properties that are, to use his words, "peculiar" and "strange" and are also different from natural structures. For the sake of clarity, the properties Bhaskar attributes to social structures are listed and numbered together with appropriate quotations as follows:

> *Men do not create society. For it always pre-exists them.* Rather it is an ensemble of structures, practices and conventions that individuals reproduce or transform. *But which would not exist unless they did so* (1979: 45-46).

1. Society (social structures) pre-exists people.
2. Human activity is a necessary condition for the existence of society (social structures).

> *Society, then, provides the necessary conditions for intentional human action,* and intentional human action is a necessary condition for it (1979: 46).

3. Society is a necessary condition for intentional human action.

> Nevertheless in employing a causal criterion to establish the reality of social facts, Durkheim observed perfectly proper scientific practice. Though it must be noticed that we are here dealing with a most peculiarkind of entity: *a structure irreducible to, but present only, in its effects* (1979: 50).

4. Social structures cannot be reduced to (explained by) their effects (human activity).
5. Social structures are present only in their effects (human activity).

> What does distinguish it (society from natural objects of scientific inquiry) is that *not only can society not be identified independently of its effects, it does not exist independently of them either.* But however strange this is from an ontological point of view, it raises no special epistemological difficulties (1979: 57).

6. Society does not exist in the absence of human action, or when action does not exist.
7. Society cannot be identified when it does not exist.

> If social structures constitute the appropriate mechanism-analogue (of natural structures) then we must at once register an important difference - in that, unlike natural mechanisms, *they exist only in virtue of the activities they govern and cannot be empirically identified independently of them* (1979: 47).

8. Social structures cannot be empirically identified *in the absence* of the activities they govern.
9. The existence of social structures is dependent (a function of) on the activities they generate.

(Italic type in the above quotations does not appear in the original text.)

In order to evaluate Bhaskar's model, it is necessary to subject to careful analysis the properties he attributes to social structures. To anticipate the arguments that follow, statements (4) - (8) are all true but not because they only refer to the properties of strange and peculiar social structures but because they refer to and define all generative structures; natural, artefactual, and social. It does not appear that statements (1) and (9) can both be true. They can, however, both be false. Furthermore, if statements (3) and (9) are both true, then change in social structures does not seem possible. An extended discussion of the above points is required in order to provide a foundation for a critique of Bhaskar's model and its fundamental premise that society pre-exists people.

Two kinds of questions can be asked about structures; what are their properties and what are their forms, and these questions refer to different aspects of the structure's existence or reality, the kind of thing it is. Properties of a circle include that it is a space bounded by a line and that its area is πr^2. The form of a circle refers to its formation, that is to the conditions or rules of its formation. To say that the form of a geometric structure is a circle is to say that a line must be drawn in a particular way according to a set of rules. In the case of a circle drawn by a computer, the set of rules that generate or form the circle is the program. Programs or, more generally, generative mechanisms are also structures with properties and form. The rules that govern the form of a program include the syntax of the computer language used, the kind of computer, etc. The properties of a program or generative mechanism are the rules it generates to produce an effect; the rules that determine where points are drawn in a two dimensional plane to effect a circle. Structures constituted by bundles of rules, such as programs and recipes, whose effects are to generate and govern the form of action could be called process structures or functional structures.[1] The importance of distinguishing between structures and functional structures is that the latter only exist when they function. When we swap or purchase "programs" and "recipes" what we are giving or receiving are not programs and recipes but representations of programs and recipes. If we have a program that will draw a circle on a screen or paper, we can distinguish three entities; a circle, a program that generates the circle, and a representation of the program stored in some medium such as paper, magnetic tape and so on.

The properties that Bhaskar attributes to social structures are not peculiar, ontologically strange, or necessarily different from natural structures. They are the defining characteristic of all functional structures. Structures that cannot be reduced to their effects (4) and are present only in their effects (5), seem fairly common-place. Apart from computer programs and recipes, records and tapes, books and movies, are all functional structures that only exist when they function; that is when a program runs in a computer, a record turns with a needle in its groove, a book is read by a person. When a program, record, or book does not function, it is a representation of the structure, a virtual or potential structure. The effects of these structures when they function (a circle produced by a program, music by a record, meaning by a book), are the properties of the structures that produce them. Statement (4) is true of all functional structures such as a programs, records, and books. They can not be reduced to their effects which are circles, music, and meaning. In other words, functional structures cannot be explained by their properties. They can, however, be described by their properties (A record is a thing that produces music) and because they are functional structures, the descriptions will entail what the structures do. The important point is that although a structure may be described by its effects, it must be explained by the conditions or mechanisms that generate it, that is in virtue of which it is generated. These conditions or mechanisms are the properties of some generative structure other than itself.

Statement (5), that a structure is present only in its effects, follows from (4). This is a legitimate, if awkward, way to say that because functional structures have properties (effects), they must be present when their properties are present. What this means is that while a circle is being drawn, music is being played, and a book being read, a program (or set of rules), record, and book must be present. Natural events and the structures that generate them function in the same way. Lava from an erupting volcano or earthquakes from geological faults, quacks from ducks, bees directed to pollen sources from the dance of their scouts, and so on, are all the effects of natural functional structures. A dormant volcano is a hole in the ground that may represent a volcano but it is only when an eruption occurs that the hole in the ground becomes a volcano. Like Bhaskar's social structures, a volcano cannot be identified and does not exist independently of its effects (6, 7). In summary, a functional structure is present, and only present, in producing its effects. It is not present and does not exist in the subsequent structure that may be generated by these effects. Circles, music, larva, quacks and quakes do not contain or have as their properties programs, records, volcanoes, ducks and geological faults, respectively. To the extent that social structures are characterized by statements (4)-(8), they are no more peculiar than other functional structures. But more important, there is nothing about these characteristics that suggests or necessitates the extraordinary claim by Bhaskar that social structures, unlike other functional structures that generate effects, "exist only in virtue of the activities they govern" (9).

To appreciate the full significance of the difference Bhaskar attributes to natural structures that generate events and social structures that generate human activity, it is necessary to consider the following two statements:

(a) A structure exists only in virtue of the activities that govern it.
(b) A structure exists only in virtue of the activities it governs.

Statement (a) refers not only to natural structures but to all structures. It means that a structure's existence is dependent on the mechanisms of its formation, the conditions of its making, and is the basis for the search for generative mechanisms in scientific activity. When the generative mechanisms are understood, they constitute the explanation for the event. Earthquakes are generated by geological faults and these in turn may be generated by other mechanisms such as continental drift, and so on. Each structure down the line exists only in virtue of the activities that govern it. According to Bhaskar, however, social structures do not work in this way. They operate in the reverse direction as indicated in statement (b). It is not clear what statement (b) means and if, in fact, it means anything. Unfortunately, Bhaskar does not explain what it means. It appears to mean that a structure's existence is governed by its effects. This kind of structure seems to entail a kind of causality in which effects precede causes and a kind of reality such that the conditions that govern a structure's existence, are the same conditions the structure governs in order to exist. This seems to mean that before something can exist, it must produce some activity and then, in virtue of producing that activity, it will come into existence.

Whatever the logical, transcendental, or scientific merits of Bhaskar's claim concerning social structures, it is clear why it is necessary. If social structures are real generative mechanisms that pre-exist human action, and if human actions reproduce and transform these structures, then it is necessary to provide a means whereby social structures can change themselves.

Human actions can only produce structures whose forms are generated by the properties of such action. This action, in turn, is generated by the properties of pre-existing social forms. In other words, for social structures to change, human actions must change. To say that action transforms existing structures is to say that different actions generate different structures, or that social structures are changed by transformed actions. This raises the question of what pre-exists or what are the generative conditions of such transformed human actions. If it is argued that social structures always pre-exist and generate human action, then change in such action is as much a mystery as is the creation of social forms that are not created by people but always pre-exist them.

To say that a social form is transformed by human action but not created by human action appears to be a game of words. For social structures to pre-exist people (human action), in the sense that they generate human action (or its form), and for change or transformation to be possible, the pre-existing social structures must already anticipate the form of the transformed structure. The point, then, is simply that if social structures cannot transform themselves and require for their transformation human action, then social structures must either be created by human action or immaculately conceived. Bhaskar claims that his model "as a result of its emphasis on material continuity, can sustain a genuine concept of *change,* and hence of *history*" (1979: 47). But the material component of the continuity that sustains the change is a special kind of material: social structures that pre-exist and, at the same time, also exist only in virtue of the activities they govern. History, then, for Bhaskar, is a kind of one step forward, two steps backward affair, where the forward steps make possible the backward steps that generate the forward steps.

A genuine concept of change and history must entail a genuine conception of material continuity. Bhaskar's fundamental premise that people do not create society because it always pre-exists them has the same form as the statement, "People do not create nature because it always pre-exists them". But the term "existence" does not mean the same thing when applied to society.

Pre-existing natural structures do not require the agency of human action for their existence. It is surprising that Bhaskar offers very slender reasons to support his fundamental premise that society always pre-exists people. It is this argument that carries the burden, not only of Bhaskar's model, but also of the claims about the reality of social structures that are entailed by his model. He defers to Durkheim's argument in the following passage.

> Similarly, the church-member finds the beliefs and practices of his religious life ready-made at birth; their existence prior to his own implies their existence outside of himself. The system of signs I use to express my thought, the system of currency I employ to pay my debts, the instruments of credit I utilize in my commercial relations, the practices followed in my profession etc., function independently of my own use of them. And these statements can be repeated for each member of society. Here, then, are ways of acting, thinking, and feeling that present the noteworthy property of existing outside the individual consciousness (1966: 1-2).

The problem in the above passage is not only the incipient reification of social facts that turns on the distinction between *each* member of society and *all* members of society. The assertion, with which Bhaskar concurs, that "The church member finds the beliefs and practices of his religious life ready-made at birth", is persuasively misleading. It does, however, illustrate an important point. Church members do not find anything ready-made at birth because at birth, they are not church members and do not practice any religion. What is found ready-made at birth are not beliefs and practices functioning "independently" or "existing outside" individual consciousness but other people and their activity including church-members, sign and currency users, and practitioners of all kinds. The problem is where and how do people find beliefs and practices, how do they become church-members, language users, debtors, creditors, and professional practitioners? The point is that Durkheim's coercive social facts, and Bhaskar's enabling real social structures that generate human action, appear to be age restricted. Ready-made and pre-existing social structures seem to pass over the heads of children in much the same way that social theorists tend to gloss over the existence of children.[2] What is real for children are the enabling and coercive powers manifest in the actions of adults. No one has said it better than Durkheim.

> (1) Considering the facts as they are and as they have always been, it becomes immediately evident that all education is a continuous effort to impose on the child ways of seeing, feeling, and acting which he could not have arrived at spontaneously. (2) From the very first hours of his life, we compel him to eat, drink, and sleep at regular hours; we constrain him to cleanliness, calmness, and obedience; later we exert pressure upon him in order that he may learn proper consideration for others, respect for

> customs and conventions, the need for work, etc. (3) If, in time, this constraint ceases to be felt, it is because it gradually gives rise to habits and to internal tendencies that render constraint unnecessary; but nevertheless it is not abolished, for it is still the source from which these habits were derived ... (4) What makes these facts particularly instructive is that the aim of education is, precisely, the socialization of the human being; the process of education, therefore, gives us in a nutshell the historical fashion in which the social being is constituted. (5) This unremitting pressure to which the child is subjected is the very pressure of the social milieu which tends to fashion him in its own image, and of which parents and teachers are merely the representatives and intermediaries (1966: 6). (The points are not numbered in the original text.)

The first point in the above passage constitutes a definition of social action. Human actions (seeing, feeling, and acting) that could not occur spontaneously but are produced through the mediation of other people can be distinguished as social actions; actions whose origin lie in the biological functions of the human organism and are produced spontaneously are non-social or biological.[3] Bhaskar's argument that "The 'given' component in social action can never be reduced to zero, analyzed away" (1979: 47) is correct but not because what is 'given' are material or real social structures. From the perspective of the child, what is given and cannot be *reduced to zero* is a material other person. From the perspective of the social theorist, what is given and cannot be *analyzed away* is a definition of the term "social" which is inserted into the slot between zero and action.

The second point is that it is other people ("we") who, through their actions, "compel", "constrain", and "exert pressure" on the child to act in particular ways. Throughout this process, children's social actions are generated and regulated by other people and not by pre-existing social structures.

The third point is that the "constraints" imposed on the child by others "gradually give(s) rise to habits and to internal tendencies" that render unnecessary external constraints but, nevertheless, embody these constraints. Unlike spontaneous habits, whose origin are in the biological constitution of people, these "habits" are social and their origin is in the actions of other people.

The fourth point is that it is through education that human individuals are constituted as social beings. The social constitution of people, as distinct from their biological constitution, has its origins in the actions of other people and consists of internalized representations of these actions or social habits to use Durkheim's terminology. It is when people are socialized, already constituted as social beings, that the restrictions on social structures are lifted. Only at this point do social structures impose either their coercive or enabling powers. It is only church-members who are constrained in their actions by religious beliefs and for whom religious social structures provide the enabling conditions for their religious practices.

The fifth point is that parents and teachers act as cultural agents to fashion the child in the image of the culture in which they participate. The "unremitting pressure" of culture, exerted through its agents, is not directed at satisfying children's personal needs or what are often called psychological needs. The point is well illustrated in Bhaskar's example that the reason why garbage is collected is not the same as the garbage collector's reason for collecting it. Bhaskar's point, in this example, is that the autonomy of the social and the psychological "is at one with our intuitions" (1979: 45). Durkheim's point appears to be the opposite. Children learn the reasons why garbage must be collected and not the reasons why a particular person collects garbage. In this way, garbage collectors not only know their reasons for collecting garbage but also the reasons why garbage is collected. What is acquired through this process of socialization are not, as Bhaskar maintains, "the stock of skills and competencies appropriate to *given social contexts*" but the skills and competencies that *constitute social life* as distinct from biological existence.[4]

Durkheim's account of the role of education provides an answer to the question of how people come to occupy what Bhaskar calls "position-practices" or social roles such as church-member. In addition, it provides an answer to the question of where generative social structures are to be found. The "habits" and "internal tendencies", to which Durkheim refers, are representations of the actions of other people who initially generate the child's non-spontaneous activities by directing, regulating, constraining, and coercing the child to act in particular ways. These "social habits" have all the properties of functional structures. They are irreducible to their effects, are present only in their effects, cannot be identified independently of their effects and do not exist independently of their effects. Furthermore, like all functional structures three components can be identified; a set of effects, a set of rules or generative mechanisms, and a form of representation. But unlike Bhaskar's social structures, they do not exist in virtue of the effects they govern. As Bhaskar points out, "if social structures constitute the appropriate mechanism-analogue" of natural structures and if "they exist only in virtue of the activities they govern", then they have the following consequences for people:

> Thus men in their social activity must perform a double function: they must not only make social products but must make the conditions of their making, i.e. reproduce (or to a greater or lesser extent transform) the structures governing their substantive activities of production (1979: 48).

Contrary to Bhaskar's account, if social structures exist in virtue of the activities that govern them, then what is required is not that people in their social activity must perform two functions but that two people must function together to produce a social performance. This does not require that people deliberately or consciously direct their actions at others but only that one person's actions have consequences, intended or unintended, for the actions of another. Social life is possible when the actions of one person function as the generative mechanisms for the actions of another person. Social actions produced in this way, in turn, may serve to regulate the actions of others, or may generate social products such as books, factories, scientific theories, etc., which, in their turn, may regulate the actions of others. The relations that obtain between the social actions produced by people, and between these actions and social products, may, if they are stable or relatively enduring, constitute a form or system of relations. In the same way, the relations that obtain between points in space constitute geometric forms. But these "social structures" or sets of relations are not the structures that generate human action anymore than geometric structures generate the programs that draw them, or music the records and musicians that produce sounds, or lava the volcanoes that erupt.

As Bhaskar correctly points out, the subject matter of sociology is concerned with structures constituted by the relatively stable relations that obtain between roles, what he calls positioned-practices, and not between the people who engage in them. These social or sociological structures must be distinguished from the social structures that engage people or generate social actions. It is in regard to this distinction that Bhaskar's ontological hiatus applies. The hiatus is not between the social and the individual, or society and people. The ensemble of social structures that generate the actions people produce are as much a part of their constitution as are the structures of their biological constitution. The social structures that explain the sociologist's actions are "radically different kinds of thing" from the structures the sociologist explains. Instead of attributing different properties to these two kinds of structures, Bhaskar conflates them and attributes different properties to people and society. Emphasizing "The importance of distinguishing categorically between people and societies" (1979: 44). Bhaskar explains as follows:

> The conception I am proposing is that people, in their conscious activity,for the most part unconsciously reproduce (and occasionally transform) the structures governing their substantive activities of production. Thus people do not marry to reproduce the nuclear family or work to sustain the capitalist economy. Yet it is nevertheless the unintended consequence (and inexorable result) of, as it is also a necessary condition for, their activity (1979: 44).

An alternative conception is that the structures that govern the "substantive activities of production" (human action), are not the same as the structures produced by such activities. Thus, the nuclear family does not reproduce people to marry and the capitalist economy does not work to sustain people. The nuclear family and capitalist economy may well be the unintended consequence and inexorable result of marriage and work but they are not necessary conditions for marriage or work, and they do not govern any activities. Nuclear families and capitalist economies define certain sets of relationships. In this sense, they are structures with properties like circles and squares, symphonies and circuses. But of cardinal importance is that nuclear family structures and capitalist economic structures are not constituted by people. Nuclear families are constituted by husbands and wives or expressed differently, husbands and wives are properties of nuclear families. But the terms "husband" and "wife" refer not to people but to relations between roles, to "husband-ing" and "wife-ing". Nuclear family structures are necessary logical conditions for husbands and wives but they are not generative structures that govern substantive activities of production.

It is precisely because the social structures that generate human action are different from the social structures generated by and through action that social change is possible. By attributing to his social structures generative properties that they do not own, Bhaskar is able to clip the wings of human intentionality and ground it in the present. This is evident in his brief description of the properties characteristic of people.

> Human action is characterized by the striking phenomenon of intentionality. This seems to depend upon the feature that persons are material things with a degree of neurophysiological complexity whichenables them not just... to monitor and control their performances, but to monitor the monitoring of these performances and to be capable of a commentary upon them. This capacity for second-order monitoring also makes possible a retrospective commentary upon actions, which gives a person's own account of his behaviour a special status, which is acknowledged in the best practice of all the psychological sciences (1979: 44).

As Vygotsky repeatedly pointed out (1978), the significance of the human capacity for second order monitoring is not that it makes possible a "retrospective commentary upon actions". Rather, it makes possible a prospective or pro-presentation of potential actions and the potential structures or sets of relations that would obtain as a consequence of such actions.

Change is possible because people have the capacity to project themselves and others into a possible future and not because social structures operate backwards and retroject themselves into a pre-existing past. How people do this may not be fully understood but that people can do this is apparent to anyone who makes a cup of tea and is not surprised when it appears. In the "best practice" of the psychological sciences, intentionality is not explained by the complexity of the material neurophysiological apparatus but by the human capacity to use complex social structures, such as language, as symbolic tools to transform their representations of human actions and the structures they generate. These transformed representations may generate transformed actions and if these actions have consequences, intended or unintended, for other people, new sets of relations with different properties will emerge.

It is the very striking feature of intentionality that Bhaskar's transformational model of social activity unintentionally undermines. The human property of pro-presentation is replaced by a social property of pre-presentation. Even allowing for a world of pre-existing social structures, a history before history, any social product that could be generated by human action would entail a reproduction and the world of social structures so produced would not know the meaning of change. Such is the non-human, a-historical, static social world of ants, termites, bees, and other social insects. A structure that exists only in virtue of the activities it governs does not require any generative mechanism other than itself. As such it cannot be explained because it does not require explanation. It is an ultimate fact or first cause. Structures of this kind are the substance of theology, the essence of mysticism, and anathema to science. The myth that is entailed in Bhaskar's transformational model of social activity is a thickly disguised reproduction of a very old story: in the beginning was the social structure and people were created to reproduce its form.

Notes

1. The concept and term "functional structure" derives from Pascual-Leone's Theory of Constructive Operators (Pascual-Leone & Goodman, 1979).
2. In important exception is Giddens (1979, 1984).
3. Volosinov (1973: 34) points out that the term "individual" is ambiguous in relation to biological and social concepts.
4. For an extended discussion of the constitutive view of social life see Ingold (1986: 222-292).

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Values and objectivity in the social sciences

Johann Mouton

1. Introduction

> All social scientists, by the fact of their existence, are involved in the struggle between enlightenment and obscurantism. In such a world as ours, to practice social science is, first of all, to practice the politics of truth (C. Wright Mills, *The sociological imagination*, 1959: 178).

In his analysis of the role of values in social science, C. Wright Mills argues very persuasively that the value of truth takes on a political meaning in social research. The "politics of truth", therefore, refers to the fact that social science is per definition value laden. If politics is broadly defined as the study of power and influence or even of the balance of power in society - i.e. the study of power play - it could also be argued that Wright Mills had in mind the power play of truth and perhaps - to link up with our immediate concern - the truth of various power plays in social science. To phrase it differently: truth is not revealed, but is constructed by and through the process of practising science.

A deeper meaning of the quotation, which is borne out by the rest of his discussion, is that (the) truth is caught up in a power play and is therefore unwittingly the victim of the political interest of the social sciences. The question then arises whether there is more than one legitimate political interest? If so, what assurance do we have that the truth of our research findings will not be sacrificed to the interests of the current players in the power game? And what remains of the traditional ideal of objectivity?

These issues are addressed in this essay. It consists of four parts. In the first section I briefly review the history and main tenets of the traditional notion of objectivity. In the second section I argue that an interest in power is an intrinsic feature of all social sciences research. The third section is devoted to a discussion of the dominant traditions in current philosophy of social science with in each case an emphasis on the specific metaphor of power. In the final section I address the question of whether it is possible to defend the notion

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of objectivity in social science, while also maintaining that social science is intrinsically value-laden and political.

2. The naturalistic notion of objectivity

In his well-known introduction to the philosophy of science, Alan Chalmers refers to what he calls the "common-sense view of science" and summarizes it as follows:

> Scientific knowledge is proven knowledge. Scientific theories are derived in some rigorous way from the facts of experience acquired by observation and experiment. Science is based on what we can see and hear and touch, etc. Personal opinion or preferences and speculative imaginings have no place in science. Science is objective. Scientific knowledge is reliable knowledge because it is objectively proven knowledge (1982: 1).

Inherent to this view of science which has become part and parcel, not only of research in the natural sciences, but also of the social sciences, is, what I would like to call the "naturalistic" notion of objectivity. As I will show, the naturalistic definition of objectivity had its origins in the seventeenth century, was further developed in the Enlightenment and indeed became the accepted and to some people - even self-evident - notion of objectivity.

The central assumption in the naturalistic definition of objectivity is that research can only be valid and reliable if all subjective "variables" - all presuppositions, preconceived notions and values - are bracketed (at least for the time). The scientist must approach reality with an open mind and allow the facts to speak for themselves. According to this notion of objective research, it is assumed that there is a "distance" between scientist and the object of research in the sense that no involvement between them should be allowed since such an involvement could lead to biased and prejudiced research. The term "naturalistic" refers to the fact that this ideal of objectivity originated in the natural sciences, where the assumption that a distance between subject and object is desirable, is perhaps less problematic. As I will argue in the rest of the paper, this assumption is less plausible when applied to the social sciences.

In the beginning of the seventeenth century, the main spokesmen for the new philosophy of science - Rene Descartes and Francis Bacon - both advocated such a naturalistic interpretation of objectivity.

With reference to Descartes, Lundin writes: "If he is 'to establish anything firm and lasting in the sciences,' Descartes argues, the scientist, philosopher or critic must by 'a deliberate effort' rid himself of all preconceived notions and start entirely anew, 'building from the foundations up'. " (1985: 4) According to Lundin, Bacon shares this point of view:

> Francis Bacon stressed the need for a rigorous empirical examination of physical, historical and experiential data. The observer was to study the data before him without allowing his biases to shape the organization of the facts.-Once he had done so, Bacon claimed, the laws governing those facts would reveal themselves to him (idem: 5).

A prominent spokesman of the Enlightenment, Baron d'Holbach, followed in the footsteps of Bacon and Descartes. In the Preface to *The system of nature* which was published in 1770, D'Holbach begins with an analysis of the sources of man's ignorance:

> The source of man's unhappiness is his ignorance of Nature. The pertinacity with which he clings to blind opinions imbibed in his infancy, which interweave themselves with his existence, the consequent prejudice that warps his mind, that prevents its expansion, that renders him the slave of fiction, appears to doom him to continual error (viii).

And his reply: "to examine with rigour and impartiality the opinions he has adopted". But what are the reasons why man continues to hold fast to such blind opinions and prejudices? For D'Holbach the answer is self-evident:

> ... in short, man disdains the study of Nature, except it be partially: he pursues phantoms that resemble an ignis-fatuus, which at once dazzle, bewilder, and affright: like the benighted traveller led astray by these deceptive exhalations of a swampy soil, he frequently quits the plain, the simple road of truth, by pursuing of which, he can alone ever reasonably hope to reach the goal of happiness (ix).

D'Holbach conclude his Preface with the following sentence:

> He (the author) writes not for those who are deaf to the voice of reason who judge of things only by their vile interest or fatal prejudices: his cold remains will fear neither their clamours nor their resentments, so terrible to those who, whilst living dare proclaim the TRUTH (x).

These few quotations from D'Holbach already provide us with an indication of the main themes of the Enlightenment: the struggle between rationality on the one hand and all forms of ignorance, prejudice and dogmatism on the other hand. At another level, this is a struggle between science (as the epitome of rationality) and religion (as embodiment of dogmatism and superstition). For the purpose of our discussion, it is important to note that "prejudice" is given a very specific meaning, i.e. as that which is contrasted to and inimical to human rationality. In his blind adherence to prejudices, man inevitably falls prey to fiction. He is deceived and misled as to the true state of affairs. It is , therefore, understandable that the person who holds on the prejudice, is doomed to falsity and thus, also to unhappiness. No wonder that D'Holbach writes of "fatal prejudices". In his search for truth, man constantly fails because he remains a victim of these prejudices.

To summarize then: The traditional notion of objective research as research devoid of any subjective factors - values, interests, etc. - was developed in the seventeenth and eighteenth centuries by people such as Descartes, Bacon and D'Holbach who all shared the basic Enlightenment commitment to reason and rationality. Objectivity became to be equated with rationality and objective research with value-free research.

What are the main tenets of the value-free interpretation of objectivity as it was worked out in the twentieth century? In another paper (Mouton, 1986), I discussed this matter in more detail. I will, therefore, only suffice with a summary of the main points here.

Adherents of the value-free position, such as Max Weber and Ernest Nagel, would support the following theses:

- That all scientific activity presupposes some framework of meanings or values. Weber calls this his thesis of the "value-relevance" of science.

- That the social sciences are still capable of attaining to objectivity. The validity of social sciences research is founded on the methodological procedures which are applied *after* the research problem has been formulated and a methodological strategy chosen by the scientist.

- That social science findings should be utilized for the good of society and that moral questions are involved in the utilization of such results.

- That the primary task of social scientists is to provide an empirical, descriptive account of reality, and not to evaluate it. Stated differently, it is not the primary task of social scientists to make non-epistemic (moral, political or social) value-judgments.

- Theses [3] and [4] are only compatible if one accepts the principle of role differentiation. Although the social scientist qua *social scientist* must avoid all pronouncements of an evaluative nature, the social scientist qua *citizen* can and does of course make all kinds of value judgments.

Although this twentieth-century version of the relationship between objectivity and values is more sophisticated than earlier notions, it still shares with them the assumption that non-epistemic value-judgments should not be part of the research activity. I will argue in the following two sections (1) that value-judgments regarding social, political and moral issues are *inevitably* part of the research process and (2) that this "fact" is reflected in the various paradigms in the philosophy of social science as well.

3. Social science and power

In a recent work John Thompson defines power as follows:

> Power can be loosely conceptualized as the capacity of an agent or agents to secure specific outcomes through their intervention (or non-intervention) in the course of events (1984: 68).

In typical Aristotelian fashion, power is defined as a specific ability to provide certain effects; to accomplish change. I will follow this usage: when I refer to the *political* interest of the social sciences, I have in mind the ability or potential of social sciences knowledge to accomplish change (in people or society).

There are probably few social scientists who would want to argue that their findings should not have an impact on people and society. It seems problematic, except if we want to claim the luxury of a monastic or ivory tower-attitude, not to believe in some notion or ideal of a useful and valuable social science. The meaning of "useful" and "valuable" would vary from school to school. Regardless of whether the ideal is social improvement, intellectual enlightenment or political emancipation, the assumption in each case is that the social sciences have an effect on the *conditio humana*. Therefore, to the extent that the practice of social sciences research should be able to bring about significant change in the individual and society, we should admit to at least a minimal political interest.

However, I would like to argue for a stronger position, namely that the interest of the social sciences in power, in other words in bringing about change, is not only a desirable aim but in fact an intrinsic dimension of all social sciences research. It is intrinsic in the sense that it follows from the particular nature of social sciences research itself. The premisses of my argument are twofold: (1) that social sciences theories (social theories), unlike theories in the natural sciences, constitute an integral part of the social practices that are studied and (2) that these social theories are, of course, themselves social practices. The implications of the first premise, is that our social theorizing - as is the case with other human actions, such as cultural, religious and linguistic activities, constitutes a social practice. It is a collective, human activity with its own set of norms and values, etc. As such, and that is the meaning of the second premise, as a social practice, the practice of social theorizing is influenced by other existing practices.

In contrast to the theories which we find in the natural sciences, which deals with inanimate objects, the fact that people are the "object" of social theorizing, has two important implications: social theorizing determines and is determined by other social practices. Like all social practices (politics, language, religion), the practice of social science does not leave the people (whom it studies and investigates) unaffected. Social theorizing explains, interprets, predicts, analyzes, informs, demystifies, unravels and demythologizes human behaviour and actions and in this wav becomes part and parcel of those same actions and inevitably changes existing practices and customs.

Various philosophers in the social sciences, including Husserl, Schutz, Winch and Taylor, have elaborated on these ideas, by emphasizing the central role that commonsense descriptions and interpretations play in the lives of social actors. I refer briefly to Charles Taylor's argument in this regard.

Taylor points out a common mistake that social scientists tend to make, namely that of attempting to imitate the model of the natural sciences in the social sciences. He writes:

> But the big disanalogy with natural science lies in the nature of the common-sense understanding that theory challenges, replaces or extends. There is always a pre-theoretical understanding of what is going on among the members of a society, which is formulated in the descriptions of the self and other which are involved in the institutions and practices of that society (1985: 93).

The social practices that constitute a society - religious rituals, political behaviour, cultural customs - inevitably require self-descriptions by the participants in these practices. According to Taylor, self-descriptions should be regarded as constitutive of our social practices because they contain an (often implicit) understanding of the suitable norms for certain situations. Taylor indicates how this fact is related to social theorizing.

> In a sense, we could say that social theory arises when we try to formulate explicitly what we are doing, describe the activity which is central to a practice, and articulate the norms which are essential to it... But in fact the framing of theory rarely consists simply of making some continuing practice explicit. The stronger motive for making and adopting theories is the sense that our implicit understanding is in some way crucially inadequate or even wrong. Theories do not just make our constitutive self-understanding explicit, but extend, or criticize or even challenge them. It is in this sense that theory makes a claim to tell us what is really going on, to show us the real, hitherto unidentified course of events (idem, 93-94).

Taylor continues by giving examples of important theories that have succeeded in unmasking the obvious self-descriptions of our social practices and by demonstrating in what sense such descriptions are misleading: Freudian theories on the unconscious motives of political actors, Marxist theories on the true motivating forces which underlie economic practices, critical theories concerning false consciousness, such as Marcuse's analysis of advertising practices in modern society, etc. Taylor further discusses how theories can sometimes challenge and undermine existing practices in society. In this way theories may reveal that what have been accepted as essential distinctions or regarded as important premises for establishing a specific practice are either false or have a completely different meaning from the one believed so far.

A good example of this from the South African situation would probably be the role of recent Marxist or revisionist interpretations of South African historiography studies that indicated the role of certain myths about the Afrikaner volk as a chosen people. The report by the main committee of the HSRC Investigation into Intergroup Relations (1985), in which specific stereotypes and myths on ethnic relations were exposed as being illusionary and misleading, is also a good example of how theories or interpretations may undermine social practices by showing that the beliefs on which those practices are based, are in fact false.

Taylor rightly concludes:

> This is the striking disanalogy between natural science and political theories. The latter can undermine, strengthen or shape the practice that they bear on. And that is because (a) they are theories about practices, which (b) are partly constituted by certain self-understandings. To the extent that (c) theories transform this self-understanding they undercut, bolster or transform the constitutive features of practices (idem, 101).

To summarize the argument of this section: Because social theorizing is a social practice, it interacts with other social practices. In particular, social theorizing can influence and transform social practices through its effect (in various ways such as unmasking, demystification, explication, confirmation, legitimation, justification, etc.) on the self-descriptions (Taylor), typifications (Schutz) or lay interpretations (Hughes) that constitute and underlie such practices. From this it follows that social theorizing inevitably has the ability to bring about change in the individual and society. This ability is not merely accidental or even just desirable, but constitutes an intrinsic dimension of all scientific study of human beings. The question is therefore not whether the social sciences have the ability to change events or human behaviour, or whether the social sciences have an interest in power, but what is the nature and content of this interest in power. How can this political interest of the social sciences be explicated? Even more importantly, can a specific form of power be defended above others? In the next section I shall briefly discuss three traditions in the philosophy of the social sciences and the various ways in which they have formulated the notion of the power of social research.

4. Metaphors of power

I discuss three of the dominant epistemological traditions in twentieth century philosophy of social science. In each case the focus is on the distinctive metaphor of power of that paradigm. In the first instance I discuss the positivist tradition which has the metaphor of therapeutic or "medical" power as its central theme. Second, there is the tradition of Enlightenment in the social sciences and the associated metaphor of Aufklärung or illumination. Finally I discuss the critical tradition and the metaphor of political emancipation.

Positivism

I use the term "positivist" as a label for those philosophers of social science who believe that the aims and methods of the social sciences are identical with those of the natural sciences. A central assumption of positivism has always been that human beings and society are sufficiently similar to (other) biological organisms, to justify the equation of methodological practices in the natural and social sciences. In the analogy of society as an organism, which underlies the equation of the social and biological/medical sciences, we find the central metaphor of therapy: the aim of the social sciences is the healing of the social diseases/evils of society. And as in the case of medical practice, effective treatment requires accurate diagnoses. The diseases of society cannot be cured before and unless their causes have been diagnosed correctly. The final aim is to control diseases by generating knowledge of these causes.

The rhetoric of this kind of medical discourse, which one finds among positivists, includes terms such as the following: diagnosis, treatment, therapy, healing, disease, mental hygiene, the health of society, anatomy, dissection, etc. It is easy to show how widespread the medical metaphor of therapeutic power has been in the history of the social sciences - from Plato, Machiavelli, Francis Bacon and Thomas Hobbes up to the physiocrats in the eighteenth century. I quote from the work of Auguste Comte only. In the *Cours de philosophie positive* he writes the following about society:

> In the highly complex social organism, maladies and crises are necessarily even more inevitable than in the individual organism. But though science is powerless for the moment amidst wild disorder and extravagance, it may palliate and abridge the crises, by understanding their character and foreseeing the issue, and by more or less intervention, where any is possible. Here, as in other cases, and more than in other cases, the office

> of science is, not to govern, but to modify phenomena; and to do this, it is necessary to understand their laws (1974: 473).

Acceptance of the biomedical metaphor with its emphasis on diagnostic knowledge of causes as a precondition for the control of disease leads to the acceptance of a methodology of research which stresses experimental control, structured and replicable observation and measurement, and quantification and generalization.

The Enlightenment ideal of rationality

I use the term "enlightenment ideal" in a broader sense than the original eighteenth century usage. Traditionally, as the term suggests, "enlightenment" has been understood to refer to man's liberation from the shackles of darkness, where "darkness" has been regarded as synonymous with ignorance, superstition, prejudice, dogma, etc. Enlightened philosophers such as Hume, Locke, the French philosophes, Wolff, Kant, and others believed that human progress would only be possible if man could overcome the obstacles of superstition and irrational beliefs. And the instrument by means of which this can be achieved is human reason.

The ideal of enlightenment has always been strongly associated with the aims of science and of the social sciences in particular. In 1770 D'Holbach wrote:

> Politics ought to be the art of regulating the passions of men, of directing them to the welfare of society, of diverting them into a genial current of happiness, of making them flow gently to the general benefit of all (Quoted in Randall, 1962: 962).

Emotions weer regarded as a symbol of the irrational passions of man which are in need of rational control. In typically Socratic manner the philosophers of the Enlightenment believed that greater rationality and intellectual insight would lead to greater happiness and peace. As Cassirer aptly remarked:

> ... all these thinkers (Holbach, Rousseau, Diderot) are convinced that reason must bear the torch on the way to political andsocial betterment. The ability to overcome the evils of the state and of society can only arise from a real enlightenment, from clear insight into the grounds and origins of abuses (1951: 267).

When I refer to the "enlightenment metaphor", I therefore include that point of view in the philosophy of the social sciences according to which the primary task of the social sciences is seen as the production of knowledge and truth in order to overcome all forms of ignorance, superstition and prejudice. The aim is to give people a better understanding of themselves and greater insight into their life situations. This point of view is expressed by Wright Mills in the passage quoted above, in which he writes about the conflict between enlightenment and obscurantism in the social sciences. Elsewhere in the same work he formulates this sentiment as follows:

> In so far as he (the social scientist) is concerned with liberal, that is to say liberating education, his public role has two goals: what he ought to do for the individual is to turn personal troubles and concerns into social issues and problems open to reason - his aim is to help the individual become a self-educating man, who only then would be reasonable and free. What he ought to do for the society is to combat all those forces which are destroying genuine publics and creating a mass society - or put as a positive goal, his aim to help build and to strengthen self-cultivating publics. Only then might society be reasonable and free (1959: 206).

In the light of the ideal to cultivate better self-understanding and more insight in the individual and thus free him/her from intellectual darkness, the interest in power in this case becomes synonymous with an interest in the power of reason.

Critical theory

Critical theory is committed to the ideal of emancipation or (political) liberation. While positivism assumes an analogy between society and the biological organism and therefore emphasizes therapeutic power, we have seen that the ideal of enlightenment (as also worked out by the interpretative school as represented by people such as Winch, Taylor, phenomenologists,, ethnomethodologists, and others) and the associated ideal of the power of reason, have their origins in the constitutive role of human reason and consciousness. In critical theory, where *political* power is defined as the ultimate goal, the central idea is that of transformation. Man is a being that can change and free himself from slavery and oppression through production and, particularly, through labour. Whereas the ideal of enlightenment focuses on intellectual oppression (in the form of propaganda, dogma and ignorance), critical theorists, following Marx and the neo-Marxists, have drawn our attention to economic or material oppression. The task of the social sciences is accordingly formulated as follows:

> The critical model envisages that its theories will become part of social life ... through what I want to call the educative role of social theory. According to this educative conception, the function of the social scientist is not to provide knowledge of quasi-causal laws to a policy scientist who will determine which social conditions are to be manipulated in order to effect a particular goal, but rather to enlighten the social actors

> so that, coming to see themselves and their social situation in a new way, they themselves can decide to alter the conditions which they find repressive ... By offering a theory which explains why it is that they are frustrated and unsatisfied, why they are doomed to continue in this condition given their conception of themselves in their social order ... the social scientist tries to show to the actors that, as long as they see themselves as relating to their social order as they do, they will remain thwarted and repressed. (Brian Fay, Social theory and political practice, 1975:103)

A central assumption of the critical approach to the social sciences, is that man is essentially in a state of alienation. Social theorizing can only be successful to the extent that it manages to help the individual to overcome this alienation, oppression and exploitation. Unlike the supporters of the ideal of enlightenment, the critical social scientist is not content with knowledge that leads to greater insight and self-reflection. The critical social scientist feels that, in addition to and on the basis of this improved insight into the individual's life-world, the social actor should be guided to change those conditions and structures that constitute his/her alienation and oppression.

With these brief comments on the three major philosophical paradigms in current philosophy of social science, I conclude this section. I have argued that an interest in change and power is intrinsic to the practice of social theorizing, even if the specific content of the notion of power differs. Thus, we discussed the positivist tradition and its emphasis on therapeutic power, the interpretivist approach and its commitment to the enlightening potential of human reason and finally, the neo- Marxist ideal of emancipation and liberation - ideals that are rooted in the basic metaphor of political change and transformation.

5. Is the notion of objectivity still meaningful?

It would be quite understandable if someone were to conclude that all of this makes the attainment of objective social research, as defined within the naturalistic paradigm (section 1) well nigh impossible. It seems obvious that one cannot both accept that social research is intrinsically political or value-laden and that it can be objective. However, this is exactly what I would want to argue: the ideal of objectivity and the notion of the political dimension of social science are in fact compatible. The key to the solution of this apparent contradiction lies in the meaning of objectivity which one accepts. I subsequently discuss the notion of "objectivity" in some detail and then continue, with a South African example which illustrates how a marriage between objectivity and "political social research" is possible.

Let me begin with a definition: Most people would agree, I think, that it is the overarching aim of all social research to produce valid findings, i.e. findings which are as true a representation of social reality as possible. Objective research, then, is defined as research which follows a methodology which will maximise the attainment of valid findings. Stated differently: objective research is research which meets the criteria of validity. On this view, "objectivity" is first and foremost a *methodological* criterion, i.e. it refers to a specific quality of the manner in which research is done.

What are these criteria of validity? Elsewhere (Mouton & Marais, 1988) I have discussed them in more detail. I will suffice with a brief discussion here.

Theoretical validity

This refers to the validity of the theory or interpretation used in a particular study. The question is: what is the scientific status of the theory? Is it logically consistent, does it have a wide range of applicability, what is its explanatory and predictive potential and is it supported by a strong tradition of empirical research?

Measurement validity

In empirical research, the social scientist usually has to employ certain measuring instruments such as scales, questionnaires, interview schedules, etc. Measurement validity refers to the validity of such instruments, i.e. do they basically measure what they are supposed to measure, are they stable and consistent over time and within different contexts, etc.?

Reliability

This criterion refers to the reliability of data-collection in the social sciences. Many factors can lead to the data being collected being unreliable, such as whether the data sources themselves are authentic and representative, whether the actual processes of gathering the data ensure reliability and accuracy, etc.

Inferential validity

Even if the aforementioned criteria are met, studies sometimes falter because the final conclusions drawn are invalid. Inferential validity refers to the validity of the inferences we draw from our data. The basic criterion is that our conclusions must be supported by sufficient and relevant evidence.

To summarize then: A study in the social sciences is objective if it meets the criteria of validity as outlined above. On this view, objectivity is a characteristic of the research process and the methodology followed, not of the researcher. Only if one defines "objectivity" in this way, does it become possible to talk about objective research being done by social scientists who have different personal values and interests. The fact that different studies in the social sciences will embody different values and therefore serve different interests in society, in itself doesnot preclude objectivity as I have defined it. Because no matter what the values are, no matter what the practical or even political aims of a study, it still has to be evaluated in terms of the criteria of validity.

Let me give two examples of social research which clearly embody different values and which have served different interests in society.

Both studies concern race relations in South Africa. The first study appeared in 1947 with the title *Justified racial separation* (Regverdige rasse-apartheid) with Geoff Cronje as author (the then professor in sociology at the University of Pretoria). The second study appeared nearly 40 years later as the main report of the HSRC's Investigation into Intergroup Relations, entitled *South African society: Realities and future prospects*.

The central arguments of both studies will be briefly summarized. The main argument in Cronje's study is the following.

Premise 1: Racial contact leads to racial integration and racial conflict.
Premise 2: Racial integration is unacceptable

Conclusion: Racial contact should be eliminated through a system of racial separation or apartheid.

Cronje refers to various kinds of evidence which supports his premisses. With regard to P1, he cites certain census statistics which show that there were on average 99 interracial marriages between 1941 and 1945 as compared to an average of 79 per year for the period 1932 - 1936. He also invokes the argument that the aversion to miscegenation is not equally strong amongst all whites; it is especially the case that contact between lower class whites and higher class blacks might lead to more miscegenation in the future.

With regard to the second premise, Cronje gives three reasons why racial integration is unacceptable:

1. It leads to the "infiltration" (insypeling) of non-white blood into the white race which can affect its purity.

2. Miscegenation leads to recessive heredity, i.e. certain traits of the parents (like the colour of the skin) will not appear in the children but in the grand children.

3. Racial integration leads to certain physical, spiritual and social disorders.

Cronje's conclusion: Apartheid is a sociological necessity. Near the end of his study, he goes even further claiming that racial separation is also a Christian necessity, when he writes: "The Bible requires that there will be national, social and religious separation (1947: 105)."

The main argument of the HSRC Study is as follows:

P1: The factual situation today is that the South African society is characterised by racial conflict and serious structural stress.

P2: Four specific problems which are basic to the relations situation, viz the institutionalization of ethnicity, one-sided absolutization of group interests, institutionalized inequality and the isolation and insulation of groups, i.e. separation can be regarded as the primary causes of the current conflict.

C: The ideal of constructive human relations can only be attained if the factors which underlie this conflict and tension (such as institutionalized racial separation), are removed.

More than 100 researchers amassed wide-ranging data in 20 disciplines as part of the Intergroup Study. And time and again it was shown that South Africa is indeed a divided and highly polarized society. In spite of any good intentions which the original exponents of racial separation might have had, it is a fact - which has been proven beyond any doubt by this research - that the system is experienced by the large majority of South Africans as discriminatory and that it has led to experiences of relative deprivation amongst the black population. In short, the legitimacy of the current system is now being questioned. The authors of the main report came to the conclusion that constructive human relations will only be realized in South Africa if such basic conditions as freedom of association, equal opportunities, the right of all individuals to participate in public decision-making, the recognition of the dignity of all human beings, etc., are met.

Is it possible to assess whether these two studies are in fact objective research or not? I think it is in principle possible and although I will not attempt such an assessment here, let me indicate how this could be done.

Let us begin with the application of the criterion of theoretical validity. In terms of Cronje's study one would have to ask how valid are his claims that racial contact leads to racial integration and racial conflict. There are a number of theoretical positions, as well as substantive empirical research, which seems to support the opposite - i.e. more contact (especially at an early age) leads to less conflict. The same question should be posed of the HSRC study: What are the status of the theories of relative deprivation and reference groups which are extensively used in the research?

With regard to the criterion of reliability, one should ask of Cronje whether he has checked the reliability of the data (especially the census statistics) which he cites. And again the same applies to the HSRC studies: How reliable are the data which were gathered through the various sources used in the study? And finally, one should evaluate both studies as to the validity of the conclusions drawn.

Where do values or political interests fit into this picture? Well it is clear that different (political) interests form part and parcel of these studies. And irrespective of whether it had been the intentions of these studies, the results of both were used by different groups in society as supporting their political ideals. It is therefore true to say that the differences between the two studies as reflected in their conclusions and recommendations are not merely differences in interpretations of the same social reality. These differences reflect fundamental differences in paradigms or intellectual discourses. The ideals of racial purity, the idea of guardianship and of national unity form the core of Cronje's discourse, while the HSRC study embodies the very different ideals and values of freedom of association, the right of all individuals to participate in decision-making at all levels of government, equality, etc.

The point which I want to make then is that it is possible, at least on analytical grounds, to make a distinction between these two aspects of social research, i.e. the paramount criterion of objectivity which all social scientists have to accept, but also that values and political interests are intrinsic to social research. A very good example of what happens if these two aspects are conflated is to be found in the way in which different newspapers responded to the HSRC study:

> Coming as they do from a body with impeccable credentials, untainted by party political affiliations, the Government must give urgent attention to the HSRC's wide ranging findings ... [The Argus, 2 July 1985]

> A word of appreciation is in order for the scientific manner in which the HSRC has conducted its investigation into intergroup relations ..... [Die Vaderland, 2 July 1985]

> The HSRC has allowed itself to be misused for party political objectives at the cost of the taxpayer .... This is an attempt at a pseudo-scientific justification of the National Party's betrayal of its principles and its shift to the left side of the political spectrum [Afrikaner, 10 July 1985]

The mistake that these newspapers make, is to assess the study only in terms of whether the conclusions arrived at are compatible with the editorial policy and not to ask whether this is good, objective science. One might excuse newspapers for doing this. It is, however, inexcusable that certain academics also continue to follow this practice.

6. Conclusion

First, social research has always been and will continue to be intrinsically value-laden and political in nature. This is a desirable state of affairs because it reflects the central concern of all social research, i.e. to improve the human condition. Differences in the definition of this political concern have always been part of our intellectual tradition: some people have stressed the notion of therapy, others have emphasized intellectual enlightenment and still others - more recently - have accentuated the notion of emancipation or liberation. These are all, as I have stated, admirable goals and - I would venture to state - all represented in current social research. The second point of the paper is that it remains meaningful to postulate objectivity as a methodological criterion for social research. The fact that we admit that social research inevitably involves values and interests, should not deter us from insisting on objectivity and validity as the paramount criteria of good research.

A definition of the goal of social research which I recently came across, is perhaps an appropriate conclusion to this essay:

> The primary intellectual aim of the humanities and social inquiry, quite generally, is to help us to realize what is of value to us in our personal and social lives. What ultimately matters is personal and social progress towards enlightenment and wisdom: all academic progress is but a means to this end [Nicholas Maxwell, *From knowledge to wisdom,* 1984: 73]

It is my contention that this objectives can only be realized if we keep a sensible balance between the role that values and interests play in social research and the ideal of objective and valid research.

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Recent trends in specific disciplines

4

Recent trends in psychological methodology

Peter du Preez

A preoccupation with methodology seems to me to be rather like a preoccupation with the meaning of life - one can only expect to receive the answer in German. (The French are also doing very well these days.) Good methodology follows good solutions to good problems. It is the function of methodology to discover why our solutions are so good when they are good and so bad when they are bad, just as it is the office of intellect to claim territory already conquered by intelligence and of snobbery to turn new money into old. Once problems have been solved, it can be quite interesting to check the etiquette of the solution; to find clothes to fit psychoanalysis, to dress ethology in the suit of a well-designed experiment. We concentrate on methodology when there are too few good methods and good problems, just as we concentrate on etiquette when manners are in decline.

To succeed, this paper would have to show how psychologists have solved interesting problems in recent times and to justify the ways in which they have solved these problems. There are interesting problems and interesting proposals, but there is no consensus about what these are, as you can easily discover by asking a group of competent psychologists at tea: "What are the hot issues, the really central problems, of psychology?" The hot issues according to one are of passing interest according to another.

It is very likely, therefore, that the trends and issues which I shall raise will seem to many to be wide of the mark. The issues are:-

(a) The problems of publication in psychological journals; and
(b) The relations between macropsychology and micropsychology.

The problems of publication derive from methodological assumptions and professional etiquette; the troubled relations between macropsychology and micropsychology derive, partly, from attempts to reject a "folk psychology" of

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beliefs, desires, selves, intentions and plans in favour of more abstract theoretical constructs, and partly from attempts to be more realistic about the conceptual nervous system. In other words, though it may appear that we plan our lives and are captains of our souls, we don't and aren't. The frequently used analogy is that the sun appears to rise but doesn't. This is not new. Psychoanalysts and behaviourists rejected the surface appearance of the unitary agent deliberately planning actions in favour of the theoretical constructs of cathexes, instincts and subsystems of the person such as ego, id, superego on the one hand; and stimuli, responses, and conditioning on the other. What is new, what comprises the "recent trend", is some metaphorical skirmishing and the incorporation of some new machinery, viz. symbol machines (programmed computers) and connection machines (massive parallel self-programming networks).

Let us now turn to the first issue, that of publication.

Publication

What are the characteristics of the major journals?

Their first characteristic is that they are seldom read, except by those who wish to write for them; secondly, they systematically distort knowledge, as a deliberate policy; thirdly, when not misleading they are often trivial; fourthly, only incompetent practitioners rely on the research they publish; fifthly, the experimental designs they employ frequently make psychological assumptions which experimenters would deny in rational conversation; sixthly, their main function is to establish professional credentials; and seventhly, all this is perfectly normal in a profession maintaining its dignity while selling its virtue.

We could look upon journals as the graveyards of intelligence if they were not the ladders of ambition.

Allow me to justify some of the things I have said. In this, I shall rely extensively on Kupfersmid (1988), Gordon, Kleiman and Hanie (1978), and Taylor (1958; 1962; 1968; 1970).

The first point is that journals are seldom read except by those who wish to write for them. What we have, therefore, is a kind of perpetual motion machine. Citation studies show that psychologists very sensibly read very little of what other psychologists write (Garfield, 1972).

The second point is that the journals systematically distort knowledge. They do this by selectively accepting papers which reject the null hypothesis, since statistical significance appears to be highly desirable in spite of the obvious truth that accepting the null hypothesis is as "good" an experimental result as rejecting it. Yet the APA Publication Manual (1983) tells us that the "reporting of negative results" is a "defect" in articles submitted to journals. What of conjecture and refutation? What of Wason's (1960) important illustrations of the significance of looking for disconfirmation of hypotheses? Kupfersmid (1988: 637-8) draws together evidence which reveals that very few experimenters (6%) would submit an article in which the null hypothesis was retained. Their reasoning is good, since there is little chance of publication. About 95 percent of articles published in major journals contain "statistically significant" results.

In listening for the whisper of a signal against the background of chaos, we report only those who imagine that they hear the signal and ignore those who don't. For every "significant" experiment we read about, there may be a dozen which show no relationship at all. This systematically distorts the truth. It leads us to suspect that most of our knowledge is fragile. How many psychological experiments can be replicated with confidence? In some ways, this is like accepting only the reports of psychotics with strong delusions and rejecting the denials of those who claim to be normal.

The third point is that journals tend to be trivial when they are not misleading. Gordon, Kleiman and Hanie (1978) present empirical evidence "which suggests that much of the research literature in industrial-organizational psychology is material already familiar to those without training in the field and hence, that this discipline appears preoccupied with commonsense matters" (:894). They found that persons without psychological training are able to "intuit the findings of recent research in industrial psychology" as well as those with training in industrial psychology can. Does this surprise anyone? The only non-obvious findings tend to be wrong.

And fourthly, the implication of the above is that only incompetent practitioners rely on research published in the journals. It is reported that "when psychologists are requested to rank order the usefulness of informational sources to their practice, research articles and books of empirical research are consistently rated at the bottom of the scale" (Kupfersmid, 1988 : 635). This is especially so when the dominant methodology stresses group results while the practitioner must understand individuals (Van Strien and Dehue, 1985).

The fifth point is that the experimental designs we employ often make assumptions that no one would defend in rational conversation. Taylor (1958) asks the rhetorical question: Would Newton have discovered the laws of motion by correlating data from many solar systems? General laws are able to predict differences as well as similarities; merely lumping together different cases will conceal relations. Can we hope to discover general laws of human psychology by pooling data from many different people who, as often as not, construe the situation differently? At best, we may discover trends and weak correlations. We simultaneously reject the view that subjects are identical and pool the results obtained from them. Why do we do this? Because we still can't take seriously the problem of designing general theories to predict individual differences. Even a theory of individual psychology, such as personal construct psychology, is frequently assimilated to the interchangeability model. The experimental model advanced by Skinner, that it is a confession of theoretical weakness to have to use more than one subject to demonstrate a result, is ignored. A recent trend, therefore, but one which has been going on for decades, is a retreat to Aristotelian science. It was not for nothing that Nisbett advised young writers to concentrate on non-controversial areas which are easy to test by standard experimental designs (cited in Kupfersmid, 1988 : 635). In fact, the lower the information content of an article, the greater are its chances of publication. Garcia's exclusion from the leading APA journals is a good illustration of this point (Lubek and Apfelbaum, 1987).

Sixthly, we can speculate that professional publication performs an important function, or it would not be so energetically pursued. This function is to create a thicket of mysteries into which only the enlightened can penetrate. In other words, it separates those who can lecture and those who can charge R80 an hour for their services from those who can't. This is why those who help to add to the pile of mysteries are well rewarded.

Is this an awful conspiracy? No, it is perfectly normal. Insiders must have either mysteries or truths to separate them from outsiders. Sometimes truth will come out of mystery. Sometimes mystery turns to magic. In practice, we may need a little of both.

Let me revert, in conclusion, to the question raised a little earlier. Should we run out into the streets confessing that the journal literature is seldom of use? This is what Blight (1987) urges on those who wish to untie the psychological knots of nuclear confrontation (and perhaps any other kind of confrontation). Needless to say, the psychologists weren't having this. They urged that their theory was good, if only someone knew how to apply it. What I suggest is that we train our students as rhetoricians. That's what they have to learn to become, anyway, but most of them learn it so badly. How else does one negotiate? How else does one persuade others to want what we want; and even more, to want for themselves what we want for them, and best of all, to want for us what we want for ourselves? Aren't these professional skills worth learning? The alternative is to want what others want, which is a position of very little power and bad for the profession.

What should be done about journals? Most of them might continue as they are, but let's imagine setting up one or two which might reverse the relationship between high probability of publication and lack of information. Let's imagine a journal which is packed with information. What are the prescriptions?

Firstly, it would do no harm to publish controversies of high quality. Articles and peer reviews should be published as a matter of course, where these are of high quality, the way that *Behavioral and Brain Sciences* does. Psychology is controversial. Articles which do not stimulate debate may not be worth publishing. At the moment, a negative critique is sufficient to kill an article. What I am suggesting is that a negative critique may merely be the start of a debate. We need editors who can distinguish.

Kupfersmid (1988) suggests that, in order to overcome the bias in favour of rejecting the null hypothesis, editors should review articles for publication without knowing the results. Papers would include a section that outlines the statistical procedures to be employed in analyzing each hypothesis. This proposal has several advantages. Firstly, papers would be evaluated by the quality of the questions they ask and the methods they use. Theory would be the focus of attention. Secondly, papers would have an equal chance of acceptance whether they accepted or rejected the null hypothesis. Thirdly, replication studies might be encouraged, especially negative studies. The result of all this is that the systematic bias which leads us to discover relations when there aren't any would be eliminated.

The disadvantages of Kupfersmid's proposal are, firstly, that authors might submit proposals for studies which have yet to be carried out; and, secondly, that there might be a flood of studies which apparently discover "nothing". However, changes in criteria of significance could take care of that, as we come to judge studies by the quality of the problem they investigate rather than by the "significance" of the data. Psychology could do with many more thought experiments integrated into theory papers. Such thought experiments, critically evaluated, could be selectively carried out by several laboratories to obtain reliable findings. A large number of laboratories converging on a smaller number of theoretically significant findings could be very fruitful. We already have something like this in multicentre research, but we have little interest in developing experimental models by critical debate, as yet. Psychology might become a witty and perceptive science, uncluttered by the swarms of "facts" which clog the mind and the journals. One can test the emptiness of our knowledge by asking honours students to state, briefly, what they regard as the most reliable and useful generalizations they have learnt. Their knowledge is seldom organized in that way. Do we ever distinguish carefully between the exact statement of a theory (in a paragraph or two) and its application to a problem? This is what we should be encouraging.

As Taylor said: "What psychology needs is not statistical laws but scientific laws, that is, statements, expressed in general terms, that can generate specific predictions when specific values are assigned to the general terms" (1958 : 106).

I shall leave our problem of publication there, and turn to the relations between macro and micropsychology.

Macro and micropsychology

The problem can be stated quite briefly in this way. At the micro level, it seems that the best account of the nervous system is connectionist. That is, the system changes by changing its network of connections. At the macro level, on the other hand, the best fit seems to be achieved by programs of explicit rules acting on explicit symbols. That is, we model the cognitive system as though it were a von Neumann computer. This symbolic model may approximate folk psychology by containing semantically interpreted symbols, or symbols with content. On the other hand, sophisticated models may operate on syntactic strings - the so-called syntactic theory of mind (Stich, 1983) which is directly based on Chomsky's separation of syntax and semantics in the late fifties.

At a still more macro level of analysis we have the normal accounts of psychology, in which people think, believe, hope, intend, act, discourse and so forth, as selves rather than as programs or connections.

What are we going to do about these different accounts? The first possibility is, of course, that all or some of them are misconceived and that good psychological theory has still to be constructed. Some would claim reality for only one level, e.g. the micro level, and reject the other. The second possibility is that both micro and macro theory represent real levels, but that the macro level emerges from the micro, the way action emerges out of muscle twitches. To use Harre's (1979) account, the meaning of a muscle twitch is the movement it contributes to; the meaning of the movement is the act it contributes to; and so forth. A third possibility is that the relation is one of refinement, as between Newtonian mechanics and quantum physics. Newtonian mechanics, although not the truth about the world in its finer details, is not only good enough, but essential for describing macro properties of the world. In the same way, though the cognitive system is best conceived of as a subsymbolic network in its actual workings, talk of intentions, plans, beliefs, wishes and desires remains the best way we have of understanding human beings acting in society, despite the drubbing from the Churchlands and others.

The methodological argument can get fierce, and the degree of fierceness is directly proportional to the emptiness of the argument, which is why there are so many wars about nothing at all. The first thing to do, then, is to look at the problems that are being solved by micro and macro approaches. Let me start with the micro level and then go on to the macro, with the understanding that it will be difficult to say anything conclusive about either.

The micro level : Symbol machines, connection machines, and possibly real objects of mind

Symbol machines

Symbol machines run programs of ordered transformations on symbols. A realist theory incorporating these assumptions would adopt the view that cognitive processes are algorithmic and that thought is "purely a matter of symbol manipulation". In addition, the mind is "disembodied", since programs run without bodily experience (see Lakoff, 1987 : 339-40).

When one considers the activity of the perceptual system in adapting to the wearing of inverting spectacles, or in locating the source of sound in an anechoic chamber, the plausibility of the above two assumptions, which are dominant in current cognitive psychology, must be regarded as very low indeed. Adaptation of the perceptual system is very unlikely to depend on symbol manipulation by rules and most certainly occurs in the embodied mind. Ashby's *Design for a Brain* (1952) made it quite clear that the multistable system of connections he was envisaging included variables in the entire system of the "body" of the machine and the environment. Taylor, using Ashby's theory, also made it clear that the mind is not in the disembodied brain, or even in the nervous system, but is an emergent feature of a system of behaviour reaching into the world.

Approaching embodiment from a different angle, Lakoff (1987) suggests that metaphors are both informative and possible because we are embodied in certain ways. (In Popper's words, the eye or any sensory organ embodies a "theory" of the world.) When we study systems of metaphors, such as the metaphors of anger or lust, we see that they are closely related to bodily experience. Anger is physiology. In it, you lose your cool, you burst a blood vessel, you foam at the mouth, you wrestle with your emotions, you turn red, you do a slow burn, you get hot under the collar, you have heated arguments, you see red, your blood boils, you stew, you seethe (Lakoff, 1987: 380-415). You are a body in a certain condition, as Sartre (1962) would say.

Colour perception, our knowledge of emotions, our preferences for certain experiences, can only be studied as embodied knowledge. We need to look at their evolution, at the operation of epigenetic rules which bias our learning in certain ways, at the complementarity of ontogeny and philogeny.

Why did the symbol machine become popular?

The first reason for the popularity of symbol machines is that they are "good to think with" (to copy the expression Turton (1980) used in referring to the Mursi system of naming colours by cattle). The more clearly we understand the workings of symbol machines, the more clearly we seem to understand the workings of mind - if the mind is a symbol machine. If the mind is not, then we may be misleading ourselves very thoroughly and seductively.

The second reason for using symbol machines as metaphors of mind is historical. The model of language introduced by Chomsky (and others) in the late fifties was perfectly assimilable to the symbol machine. This model of language not only separated syntax from semantics, but appeared to give syntax a precise, mathematical form. Since this model grew out of work on computer languages, this is hardly surprising, but the effect was overwhelming at the time. The aim of syntactic theory was to define the set of grammatical elements and their rules of transformation, and it was almost certain that a semantic theory of the same kind would follow, once "knowledge of the world" had been separated from "knowledge of the language". The theory of language sustained by these analyses, with its sharp distinction between competence and performance and its clearly formulated set of symbolic operations was used to demolish Skinner's *Verbal Behaviour* in Chomsky's 1959 review. This syntactic approach plays an important role in recent philosophy, as in Stich's (1983) theory of mind.

The third reason for embracing the symbol machine metaphor was that a level of representations could now be formulated. Mentalism was defined as an approach which operated at this level and which did not reduce every account of behaviour to s and r elements, whether fractional, mediating, external, or anticipatory. Having a theoretical tool to work with is the most important methodological imperative. If we don't have geometry, we use figures in the sand; if we don't have figures in the sand, we use our fingers. As we climb the methodological ladder, we take notice of the imperfections of the theoretical tools we leave behind. Psychology is a rusting heap of discarded metaphors. The symbol machine metaphor may be about to join the heap, for the reasons given above. Though it is useful to test theories, it is not useful to conceptualise them. It neglects the embodiment of knowledge, the ways in which knowledge is affected by psychodynamics, by sociodynamics and by culture. It is poorly structured to model subsymbolic adaptive processes, such as the adaptations of perception, kinaesthetics or aesthetics for that matter. Human knowledge is based on a system of presymbolic or subsymbolic responses to the world. I have already mentioned the way that metaphors of anger reveal our embodiment; let me give examples of the way that lust is embodied. We find that lust is hunger (he is sex-starved; she looks luscious); lust is animal (he's a wolf; she's a bitch); lust is heat (he's burning with desire; she's cold); lust is madness (he's crazy about her; she's delirious); lust is a game (he'll score; she'll lose); lust is war (he's well-armed; she'll surrender); lust is a physical force or reaction (we were drawn to each other; she's devastating) (Lakoff, 1987). And many more examples of the system of desire can be given. Concepts interact emotionally and experientially because we relate to the world in ways determined by our biology and our culture.

Symbol machines are useful to think about representations, about the possibility of different discourses running in the machine like different programs, and about partly joined operations. These are the things that were persuasive in the past, but they are no longer the problems in the foreground.

It may turn out that modern cognitive psychology is, by and large, a barren subject based on an approach at least as faulty as that of Clark Hull in the thirties and forties. Because it is based on a faulty image of mind, it can reach no correct view of its subject, though it may turn up the odd fact. The boredom of students taking the subject reflects good sense. They know a Dodo when they see it. This is, I suppose, an important methodological point.

A recent trend, embryonic as yet, is the development of subsymbolic or connectionist models. Is this a return to the efforts of people like Ashby in the fifties - attempts described by Minsky and Papert (1968) as "fearfully simple"? No. In fact, we should take warning from the start that many of these models are as disembodied as symbolic models are. Yet they may be more plausible. Let us examine one recent attempt, discussed in the 1988 volume of *Behavioral and Brain Sciences*.

Connection machines

An alternative to the symbol machine approach to mind is the connection machine approach. According to this approach, our problem is to see how, starting with a relatively unorganized brain or nervous system, connections of an adaptive kind are formed. It may be asked why we should attempt to assimilate mind to yet another machine? The reply to this is that machines are the best things we have to think with. Since the eighteenth century, there have been persistent attempts to assimilate the human mind to the best machinery available. As the machinery improved, so our understanding of the human mind seemed to improve. At first, human beings were rather remarkable automata, then complicated telephone exchanges, and finally symbol machines or connection machines.

The methodological point is that this attempt may be entirely misleading. It is part of the "spirit of the age", part of the desire to discover the algorithms or programs of mind. To the extent that this is misleading, our methodology will lead us to systematically wrong conclusions about mind.

Let us continue, though, with an account of connection machines. They are subconceptual and do not depend upon the manipulation of symbols for their operations. "Connectionist models are large networks of simple parallel computing elements .... Much of the allure of the connectionist approach is that many connectionist networks *program themselves*, that is, they have autonomous procedures for tuning their weights to eventually perform some specific computation" (Smolensky, 1988: 1).

Why is there an interest in connectionist models? The first reason has nothing to do with psychology. It depends on the promise of self-programming, highly adaptable machines for performing inventive work. The second reason, which has more to do with psychology, is that connectionists offer better models of the nervous system than symbol machine theorists do. The third is that a better model of the nervous system may lead to a better model of the mind. At this stage, connectionism is no more than an approach and many are sceptical of its claims.

What are the possible relations between connectionist (subsymbolic) and symbolic machines? Do we have to choose between them? Woodfield and Morton (1988) suggest three possible relations, and to these we shall add a fourth.

The first possibility is a division of labour in the brain. It may carry out computations of different kinds with different systems, using a subsymbolic or a symbolic system where appropriate.

The second possibility is "killing two birds with one stone: (Woodfield and Morton, 1988: 58). Thus, one may practice archery and add to the larder and impress a lover simultaneously. Similarly, the two cognitive processes of symbolic and subsymbolic computation may be carried out simultaneously by one connectionist set of neural events.

The third possibility is the "by" relation. One may launch a ship by breaking a bottle of champagne; one may "carry out a symbolic process by performing subsymbolic operations" (idem: 58).

It is the "by" relation that Woodfield and Morton favour, proposing that symbolic thinking is indeed an emergent property of connectionist systems and that it really exists: the human brain functions as a symbol-manipulator by being a subsymbolic or connectionist system. Perhaps the difference between symbol machines and connection machines should be first stressed in considering this point of view. In symbol machines, there really is a program written in a programming language and entered into the machine; in connection machines, there are only connections of different patterns and weights. The information is in the network and the connections. They are "self-programming" in the sense that these networks and weighted connections are altered during learning.

Yet emergence is a common enough phenomenon in biology. "Ethologists have proposed that some social behaviour in animals (display, courtship rituals) evolved out of displacement activities produced by conflict between basic instincts" (idem: 58).

One might imagine how, with the advent of language, operating with symbols becomes an essential part of the programming of the system. Self-directions of all kinds are evident, but are they simply output of a system of connections, rather than a real, separate element in the operation of the mind?

This leads to the fourth possibility, which is that the symbolic and subsymbolic are not levels, but different paradigms of cognitive modelling. Nor are they separate systems which can connect or disconnect, but different modes of explanation. The different approaches may be analogous to the approaches of Newtonian mechanics and quantum mechanics. Quantum mechanics is supposed to tell how things "really are", but Newtonian mechanics is essential for accounts of the world at the macro level. In a strict sense, if the microtheory is right, the macrotheory is wrong. Yet, for a large range of circumstances, the macrotheory is the best we have. In the same way, connectionism may be right. The nervous system may operate subsymbolically, by altering the weights of connections and their patterns of firing. Yet at the macrolevel, symbols are a necessary ingredient. We refer to symbols which represent intentions, beliefs, desires. Or, if we search for greater generality, we include only semantically uninterpreted, syntax-like symbols rather like those in a Chomskyan theory of syntax. The argument is hardly settled and both versions are, strictly speaking, wrong according to the connectionist account. There is no "real" level of symbol operations in the connectionist machinery of mind.

That, then, is what one of the major methodological controversies of the moment is about. I leave it to you to judge whether it is a "hot" issue or not. My own view if that methodologists can do no more than follow, like twenty maids with twenty mops, and tidy up the mess after the brawl of discovery and invention is over.

The macro level: Selves and other fictions

One thing is clear from the previous discussion: it may all be true but it is singularly unhelpful in understanding why people do what they do. It is as hopeless to advance a connectionist theory of social psychology as it is to advance a quantum theory of planetary motion. In order to explain social action, we need to develop an entirely new set of metaphors referring to persons and their acts. It may well be that the processes of this level are not "literally instantiated" at the microlevel. It may even happen that we end up with two irreconcilable theories, one for micropsychology and another for macropsychology.

An approach which is increasingly to the fore is discourse analysis. We see how easily it can be joined to psychoanalysis, to Marxist theory, or to concepts of the decentred self. But what psychological approach corresponds to discourse? In fact, there have been a number of fragmented concepts in search of an author. The metaphors of process have been script, role, game, rule, and plan. Each of these processes fits, in turn, into some sort of psychological agent.

The metaphors have been with us for a long time; they are part of folk psychology worked up a little into theory and will probably be with us a while longer. Yet, if we are in pursuit of recent trends, we have to ask whether there is a self which corresponds to discourse. The image which captures variety, skill, and an interest in persuasion is the rhetorician (Harre, 1980; Billig, 1987).

The rhetorical self

When evaluating the appropriateness of viewing persons as though they were rhetoricians (rather than scientists, actors, gamblers, bureaucrats or automata), we might consider the following:-

(1) Persons invent metaphors as well as act them.

(2) Persons are continually at work trying to persuade themselves and others of some position, truth, view. They tell jokes, stories; they make gestures. They try to impress, overwhelm, project a stance. They hold conferences. These are rhetorical occasions and strategies.

(3) Persons tie themselves into rhetorical knots and skilled rhetoricians (therapists and counsellors) are employed to untie these knots. The essence of the process is captured in Finn Tschudi's (1977) paper: *Honest and loaded questions.*

(4) The approach is reflexive. It describes what I am doing as well as what I am talking about.

A natural way to present the approach is to watch it at work. Let us start with Billig (1987) taking on some problems in cognitive psychology. He observes: "The view of Isocrates and the Eleatic Stranger, that thinking is like a quiet internal argument, has direct psychological implications. Most immediately, it suggests that psychological and rhetorical theories should be closely linked". (idem: 118).

The "internal argument" is portrayed most vividly in psychoanalytic and cognitive balance or dissonance theory. It has attracted more attention from psychologists attempting to understand breakdown than from those attempting to understand normal functioning. It has certainly attracted very little attention from cognitive psychologists, who are more concerned with coding and processing them with the possibility of argument. After all, argument plays a very small role in the operations of computers. Psychologists tend to be wedded to the ideas of balance and harmony, of fitting each input into its cognitive slot. In the words of Billig, cognitive psychologists have concentrated on logos and have neglected anti-logos, or disagreement and dissent. There are, of course, exceptions. Martindale (1984) is a cognitive psychologist (to take an example) who has drawn on the opposition of primary and secondary process thinking in his story of the development of aesthetic traditions. But what of opposing processes at the secondary level? Do we have to go outside this level for creative tension? Kelly (1955) is another psychologist who has built contradiction into his theory. Each construct has two poles which often contradict; and in the process of thinking there are contradictory needs. The first is to reduce anxiety by increasing the goodness of fit between our anticipation of events and real outcomes; the second is to avoid boredom by finding situations where our anticipations are not confirmed. We can postulate (as Devereux (1967) does in looking at the psychodynamics of cross-cultural theory), that science is a method of increasing anxiety (refutation), and of reducing it to manageable proportions (successful conjecture). Inventive scientists enjoy increasing and reducing their anxiety in this way. They avoid boredom by seeking uncertainty and then attempt to conquer that uncertainty, like mountaineers climbing new rock faces.

The particular contradiction which Billig (1987) examines is that between categorization - the central concept of modern cognitive psychology - and particularization. Those who read standard texts in cognitive psychology will acknowledge that the typical problem of cognitive psychology is seen as putting a stimulus into category, since this determines how the person reacts to it. The act of categorization is to treat "distinguishable events" as equivalent. Equally important, though, is the act of particularization, which is to notice significant differences between equivalent events. (We may note, in passing, that in Kelly's theory, to construe is to impose a grid of similarities and differences on the world. Categorization and particularization are equally important, depending as they do on a dynamic interplay of similarity and difference. Also, Piaget refers to the opposing processes of assimilation and accommodation.) But, to continue with Billig's account, we note that those who are classified as cognitive psychologists generally emphasise the process of categorization, which reduces the variety of the world to "usable proportions" (Rosch). Schemes, frames, and scripts are devices for categorizing events. Personal constructs enable us to construe replications of events.

The kind of thinking that is portrayed by emphasising categorizations is classificatory or "bureaucratic", of a narrow sort. Billig goes so far as to maintain that "social psychologists adhering to a cognitive approach sometimes tend to see all thought as being inherently prejudiced, or, at least, they suggest that prejudice arises from the normal processes of categorization, which are assumed to lie at the basis of all thought" (idem: 125). One ought, on the contrary, to realise that prejudiced and unprejudiced persons who are apparently in the same camp can be distinguished from each other by the reasoning they employ to reach their conclusions and by their willingness to continue reasoning about their conclusions. What kind of arguments and evidence are they open to?

Billig concludes that "according to the logic of categorization theory", tolerance is "strictly speaking" an impossibility, since "all experience has to be mediated by distorting categories or schemata" (idem: 127). Billig, as we see, has adopted a standard rhetorician's tactic for upsetting a one-sided picture: "... if a psychological theory concentrates upon a single principle, it often pays to reverse that principle" (idem: 130). The reverse is often as easily maintained as the original. This is not to say that the original is necessarily untrue, or that the reverse is true, but that both are incomplete.

If we reverse the basic assumption of categorization theory, which is that the organism benefits by simplifying the environment, we arrive at something equally true, which is that the organism may benefit by noticing differences. Also, human tolerance is as natural as prejudice, if we adopt the rhetorical position and cultivate it in society. Also, the assumption that we would be overwhelmed by the cognitive demands of complexity if it were not for categorization can be reversed to the principle that we would be overwhelmed by the task of reintroducing complexity into behaviour mediated by over-simplified schemes. If there are infinite ways of finding events similar, there must be infinite ways of finding them different (idem: 133). Billig's conclusion is remarkably similar to that of Kelly (1955). Categorization and particularization are not two distinct processes. We need to do both. Our cognitive processes do not function only to provide psychological stability and sameness; they also lead to argument and novelty.

Categories are often controversial. They are not "givens" or accepted parts of the social furniture in all cases. Is South Africa a terrorist state? Are our conflicts race or class conflicts? Does the equation ANC = terrorists = guerillas = freedom fighters hold? In each case some will particularize and some will categorize. Thus, one may be a patriot among dissidents and a dissident among patriots. In each case, one is distinguishing one's self (in true rhetorical spirit) from the prevailing orthodoxy and simplifying categories. Of course, there is also a time to ignore differences and to seek agreement. That is the other half of the rhetorical task.

Self and identity

The rhetoric metaphor captures many psychological characteristics of persons. The important concept of internal argument suggests a "society of mind", the possibility of several selves, the capacity to project a large number of identities and positions in conversation. On the one hand, we can detect a seminar of selves - or, to use another image, we can listen to the polyphonic music of selfhood. It is indeed a composition, but not a single voice or a simple unity. The composition conceals the diversity. On the other hand, we present ourselves as a single agent of action and are so regarded by others, except in cases where the dissociation, the multiplicity of selves, is "pathologically" transparent. When the self disintegrates it is a moral scandal and a psychological opportunity.

What is the reality of these selves? Should we insert homunculi into the person, authors into the author, scientists into the scientist? Should parent subdue child? superego restrain id? or animus squabble with anima? All of these are partial insights into the different voices, the different processes which go into the composition of self.

In an interesting paper Daniel Dennett (1988) compares the self to an *abstractum* (Reichenbach's term) comparable to the centre of gravity in physics. The centre of gravity of an object is not a physical item in the world, like an atom or a seed or a muscle protein, even though it has a spatio-temporal location and an essential place in the discourse of serious physics. It is a theoretical fiction, in contrast with what are called *illata* (again Reichenbach's term) for which real existence is claimed. Inferred entities such as atoms or cosmic rays are *illata*. It is possible to ask and answer questions about their real existence, such as: What is their mass? how fast can they move in space? do they have colour? They are analogous of the objects of the perceived world, existing independently of us. On the other hand, the centre of gravity of an object is a fiction of physics. So are many other essential properties of the world, such as "human rights" and "nation". When we change the centre of gravity of an object, say by cutting a piece off it, the centre moves from point A to point B without travelling through the space between the two. We can't ask whether the centre of gravity of an object is really an iron molecule, or whether it is pink. It is a theoretically determined and abstract point which is necessary to the explanation of physics - a conceptual reality.

Is the self a fiction of this kind? The self is an agent which is essential in the theory of folk psychology. There is a self to cause action, to take responsibility, to formulate plans, to spin stories about the world, to occupy grammatical positions such as "I" and "you". Perhaps we should introduce a distinction here. The self is a necessary fiction, a complex system of stories and processes, but it is identity which is most like "centre of gravity". Each self is capable of adopting a large number of positions or identities. Identities are positions in which persons find themselves under particular circumstances. Identities are positions from which certain kinds of utterance make sense. When persons can't find positions they suffer from confusion and may need psychological or spiritual counselling. Persons attempting to discover positions engage in defining and experimental acts of all kinds, ranging from rash adventure to psychological breakdown, ideological conversion or commitment, rational argument, stories, etc. "Project" is a term which describes the taking up of a central position, the long and arduous attempt to define it, to spell out its implications, to live it out. But "position" and "identity" are never simple. Consider the sort of social situation depicted by Fanon, in which there are several positions that can be taken up by the colonized. Should one identify with the colonizer and distance one's self from the "natives"? Should one identify with "the people" and resist the oppressors? Each of these will change the nature of the self and the position it is capable of inventing, defending and living out.

Notice how "real" the self continues to be in this discourse. My own view is that the processes of the self have a status analogous to the forces of physics. That is, we may yet identify them with some existent. What is analogous to "centre of gravity" is, quite obviously from the above discussion, *identity*. We continually project identities for ourselves and others in our talk and action. Let us now look at the fundamental force of psychology.

If there is a psychological equivalent to the concept of gravitation, which plays such a central role in physics, it is the fact that people *recognize* each other in various ways, using recognition as a general term for the ways in which people constitute each other. By this, we understand the whole range of confirmation, disconfirmation, evasion, collusion and condemnation, conveyed in the *regard* or the *look*. These are the outward manifestations, the instruments of recognition. We recognize each other as heroes or villains, as good or bad, as admirable or contemptible, as honest or dishonest. This is what makes people move towards or away from each other, what splits or unites them internally so that they become fragmented or all of a piece. It is what makes people love and hate, murder or spend fruitless years of toil to achieve pre-eminence in the hope of some future and postponed recognition (Sartre 1960; 1963).

Implications

What has been said may clarify the status of the rhetorical approach. It is one of a number of folk psychologies and it gives a reasonable account of some features of human behaviour. It is consonant with the concept of multiple selves, with much that has been revealed about the projection of positions in discourse, and of course about the complex interweaving of different kinds of discourse in our speech. To borrow Bakhtin's phrase about Dostoevsky: our stories are "polyphonic".

How could we state a formal theory of conscious mind or subjectivity which is consonant with the rhetorical position? A preliminary attempt (using Taylor [1968] as a model) would run as follows:

1. A state of multiple simultaneous readiness for different discourses determines the state of consciousness at any moment.

2. States of readiness for two discourses that are clearly distinguishable from one another determine two different forms of consciousness.

3. Changes in readiness for different forms of discourse will result in changes in consciousness.

4. Engaging in discourses which are not joined results in fragmentary and sometimes contradictory states of consciousness.

5. Discourses which have many implications for one's self are difficult to change.

This puts learning a discourse before a change in consciousness. Does this mean that consciousness is a mere "epiphenomenon"? In the sense that a particular subjectivity or position or identity of which one is conscious is an abstractum, yes. This consciousness is a state of knowledge, an indicator of a state of readiness to act. Yet while choice is undoubtedly made in "a blaze of consciousness, it is meaningless to say that it is made by some hypothetical entity or force called consciousness" (Taylor, 1970: 283).

Conclusion

The main thrust of this paper, after considering difficulties of publication, has been the split between macro- and micropsychology. One way of thinking of this split is to use the analogy of Newtonian mechanics and quantum mechanics. The former is essential in explaining large scale events even though it may not be true at the micro event level. In the same way, folk psychology may be essential in explaining large scale behaviour even though not literally true at the micro level of the nervous system.

Another way of approaching it is to say that we can look at any complex organism or system in different ways. When we adopt the intentional stance, we ask what the organism is trying to do, using terms such as plans, desires, etc. When we adopt the design stance, we ask about the structure of the system operations, about its "programming", without worrying about its physical machinery. This is the level at which psychological theorising must proceed and in fact has proceeded. We can regard psychoanalysis and cognitive psychology as attempts to understand human behaviour at the design level. Metaphors such as games, drama, and the computer are metaphors of design. Rhetoric is a metaphor of some interest at the moment, since it fits in rather nicely with discourse analysis. On this analysis, it is of little interest to psychologists what the physical systems of the body and nervous system do. The physical stance (to use the last of Dennett's (1988) terms) is for physiologists and the design stance is for psychologists.

This is, so it seems to me, substantially true.

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5

Towards a human sociology

Dian Joubert

1.

Like all theatrical phrases, the title of this paper is deliberate and intentional. It masks - transparently - an inherent accusation and an uneasy empathy: That sociology has little relevance in the everyday lives of ordinary people. *Relevance* and *irrelevance* are rational words and there are those who would argue that sociology is not - and never was - supposed to be relevant to everyday living and dying. I remember well the brazenness with which I was introduced to sociology as a student in the mid-fifties: "Sociology is concerned with social structures and systems; it has nothing to do with people." These were, obviously, the years when sociologists tried to understand and explain Parsons rather than sociology. (Which, again, is both a special case and the common practice of studying sociology instead of social reality!) Words with more feeling, would be *disappointment* and *disillusion*. Whether rational or emotional, the appreciation or otherwise of sociology, follows from a situation in which promises, expectations, achievements and failures, are very much muddled. Where, what are *perceived* as promises, expectations, achievements or failures, are at variance.

2.

Promises were made and expectations raised at the very founding of sociology. Auguste Comte committed the science of society to the humanisation of all societies. This objective, if not *raison d'etre* of sociology, was for Comte a self-evident component of the so-called "Law of Human Progress". His theory of the "Three Stages" was sociology's first Grand Theory - its boldness and scope never surpassed. The three "theoretical conditions" of theology, metaphysics and science refer not only to epistemological modes but also to the "progressive emancipation of the human mind", the evolution of social organization and the material conditions of human life. No less, it

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outlines the growth of humanism: the *condition humaine* being first interpreted as the will of God, then as being brought about by abstract forces and finally being understood as the result of specific existential conditions which can be ascertained and changed. Comte formalised the secularisation of social knowledge - showing it, in origin, use and objective, to be a very human and potentially humane thing.

The precondition for humane society, was, for Comte, scientific social knowledge. Social action beneficial to mankind would become possible once the laws of human evolution were established and the bases of social order identified. The new order would then have Love as principle, Order as its basis and Progress as its aim. The egoistic propensities to which men and women were prone throughout history, Comte believed, would then, because of enlightened social knowledge, be replaced by altruism and obedience to the command to "live for others". Was ever a science conceived with more humane ideals?

3.

What then went wrong? That would be a rather naive question. In history things do not go wrong; they go different. More often than not, ideals, plans, expectations or predictions are not realized. Even less are utopian societies. Comte's vision of a sociologically researched, informed and planned society has not been realised in 170 years of sociology. When questioning sociology's relevance for everyday living, Comte should be remembered for his conviction that scientific social knowledge is a prerequisite for a more humane society. Apart from not making the distinction between necessary and sufficient requirements, the founder of sociology also foresaw no reason why such knowledge would not be generally understood, accepted and applied.

The non-realisation of Comte's ideals can only to a limited extent be ascribed to a non-acceptance of scientific social knowledge. The real problem is both more simple and more fundamental: Comte's - and our own - conception of the nature of social knowledge and of the distinction between ordinary social knowledge (common social sense) and scientific social knowledge. These are the main concerns of this paper.

4.

Even those who have read very little sociology (before finding it irrelevant!) would anticipate the name of C. Wright Mills and *The sociological imagination* (1959).

First published thirty years ago, *The sociological imagination* is still a most incisive and empathetic critique of sociology. Mills's analysis of the condition of sociology is like a house visit of an old and trusted family doctor: The causes of the ailment and the medicine are professionally decided upon, both the problem and the cure explained in precise but understandable English - and the patient encouraged that he has much to live for.

Sociological imagination is itself an imaginative concept. It is also a most humane one.

"No study", Mills (1959: 6) writes, "that does not come back to the problems of biography, of history and of their intersections within a society has completed its intellectual journey." The sociological imagination is that social knowledge which enables men and women "to grasp what is going on in the world, and to understand what is happening in themselves as minute points of the intersections of biography and history within society" (p.7). "Personal troubles of milieu" and "public issues of social structure" are clearly distinguished:

> Troubles occur within the character of the individual and within the range of his immediate relations with others; they have to do with his self and those limited areas of social life of which he is directly and personally aware. Accordingly, the statement and the resolution of troubles properly lie within the individual as a biographical entity and within the scope of his immediate milieu.
> Issues have to do with matters that transcend these local environments of the individual and the range of his inner life. They have to do with the organization of many such milieux into the institutions of an historical society as a whole ... (p.8).

Inability to understand or cope with the connection between troubles and issues, results in alienation, uneasiness, indifference, or worse. The task of sociology is obviously not that of personal consultation in personal troubles. The task is a *cultural* one and in taking up this challenge, academic sociology is joined by other social sciences, imaginative journalism, literature and historiography. Mills argues that current sociology has largely failed in this responsibility because of three tendencies which, taken to their extremes, destroy all sense of proportions - and make the resultant social knowledge of little value to ordinary people. These tendencies are: An over-theorising of history which puts the material of human history into a formal and trans-historical straitjacket; the over-formalisation and excessive systematisation of concepts - when conceptions become Concepts; and, the tendency to cultivate research methods for their own sake - when methods become Methodology - resulting in thousands of precise but very limited empirical studies.

It should be evident that these three tendencies become problematic on all three levels of scientific thinking: conceptualisation, empirical evidence and theoretical explanation/interpretation. It is on all three these levels that a more human and humane sociology would require changes - and on all three that Mills gives some very good and practical advice. The two last chapters in *The* *sociological imagination* are on Reason, Freedom and Politics - all zones of confrontation between social conditions and individuals.

For sociology to became more human, for the ideals of the founding fathers to be realised, and for the world to be a little better place in which to live and die - for all these things to happen, Mills asks for a new sociology. Like the plea of Comte, what is required, is social knowledge of a particular quality. Mills would not insist on calling it "scientific" in the positivist connotation of the term. For that he is much too realistic about the limitations of the "scientific method" and the dynamics of social reality; much too sensitive as to the very human characteristics of social knowledge.

Mills's *The sociological imagination* is an in-depth understanding of sociology's achievements and defects and surpasses the discipline's conventional borders. His is an inspiring picture of what sociology could be for social scientists and ordinary people.

5.

If the ideas, ideals - and style - of Mills are very different from that of Comte, one could hardly find a more different third opinion than that of Nicholas Maxwell. What Maxwell argues and pleads for in *From knowledge to wisdom: A revolution in the aims and methods of science* (1984) is "a profound and comprehensive intellectual revolution affecting all branches of scientific and technological research, scholarship and education". All these he conceives of as "rational inquiry" which, based on an epistemology of aim-oriented rationalism, is to replace the institutionalized philosophy of knowledge with a philosophy of wisdom.

> Wisdom is here understood as the desire, the active endeavour, and the capacity to discover and achieve what is desirable and of value in life, both for oneself and for others.
> Wisdom includes knowledge and understanding but goes beyond them in also including: the desire and active striving for what is of value, the ability to see what is of value, actually and potentially, in the circumstances of life, the ability to experience value and realize value ...
> Wisdom, like knowledge, can be conceived of, not only in personal terms, but also in institutional or social terms. We can thus interpret the philosophy of wisdom as asserting: the basic task of rational inquiry is to help us develop wiser ways of living, wiser institutions, customs and social relations, a wiser world (1984: 66).

These quotations are a fair sample of Maxwell's style and level of argument. It is also proof of his apparent dismissal of what sociology and the social sciences have been doing for almost 200 years and - albeit modest - the progress they have made in understanding their problems in the field of philosophy of science. Ultimately, Maxwell's return to "wisdom" is a return to a vast diffuseness, which to an ordinary sociologist and an ordinary person must be more strange than were Comte's ideas 200 years ago.

For our purposes it is relevant to see that Maxwell forces the priority of personally and socially relevant knowledge to the point where this type of knowledge is equated with "wisdom"; and, that he argues, (I believe unconvincingly), that value-oriented rational inquiry has *its own epistemology*; and that this priority requires a revolution in thought - which then will itself bring about a revolution.

Upon discovering Karl Popper's *The open society and its enemies* Maxwell was most enthusiastic:

> Here was a work passionately concerned with a profoundly important problem: how are we to achieve civilization? What are the basic problems and what role does reason play in helping us to solve these problems? Subsequently, however, ... I came to the conclusion that ... Popper, along with almost all scientists, seriously misrepresented the basic intellectual aim of science (1984: 279).

Maxwell admits to an unequal reception of his message:

> During ten years of advocating the philosophy of wisdom to all and sundry, I have found that most non-academic women know what I am talking about straightaway, most academic men do not, with non-academic men and academic women falling somewhere in between.

Developments during the past ten to twenty years which Maxwell considers to be in accordance with the philosophy of wisdom are the following:

> Concern about environmental and ecological problems; awareness of the plight to those living in the third world; alternative and more appropriate technology; movement for social responsibility of science; priorities of current scientific and technical research; attention by psychiatrists to problems of living.

6.

You may not approve of my choice of Comte, Mills and Maxwell to make turbid the question of sociology's relevance for ordinary people. I must confess that I find this kind of diffusion attractive before attempting a distillation of the mixture. Also you must have noticed that I have not yet defined *human, humane* or humanity. General or technical dictionaries are not of much use if one insists on a "precise" definition. If one does not, the following phrases from an old Chambers Twentieth Century Dictionary are not to be spurned:

human: belonging or pertaining to or of the nature of man or mankind; having the qualities of (a) man or the limitations of man
humanly: in a human manner; by human agency; having regard to human limitations
humane: having the feelings proper to man
humanise: import human qualities to
humanity: the nature peculiar to a human being; humanness

Whether adjectives, adverbs or nouns, all these words refer to: *that what is considered to be characteristic to man.* Beyond this, these terms should not be submitted to formal definition. To do that would be to curtail the shifting accents that they have in everyday conversation. And to impose on the immediacy and sensibility these words have in their linkage to concepts like *personality, social condition, culture, civilised* and *history*. Formal definition would here blur rather than brighten meanings.

Of course, *human, humane, humanness* and *humanity* are also value words. Like all values they are fields of tension and suspense and become rather feckless when defined technically.

7.

Among the three of them, Comte, Mills and Maxwell have indicated quite a number of human traits of social knowledge and we must now have a closer look at these.

In what follows, I distinguish several pairs of *contrapositions within or relevant to social knowledge.* Often oppositive to one another, the one position may be judged to allow for more human characteristics in social knowledge, the other one less. Contrapositions are thus not "human" or "inhuman" - as social knowledge can hardly not be "human". What these contrapositions demonstrate, is the extend to which particular "very human" characteristics are acknowledged and accounted for in sociological work. "Work" here includes conceptualisation, the search for empirical evidence and theoretical explanation/interpretation. Behind this somewhat precarious construction, there is a suggestion: Sociology's commitment to the scientific method in understanding and explaining social reality has exacted its own price: a failure to consistently appreciate the full humanness of that reality.

8. Professional knowledge vis-à-vis lay knowledge

Also in knowledge about society and interpersonal relations the distinction between professionals, and men and women "in the street" is well established. Sociologists, psychologists, psychiatrists, social and community workers, marriage counsellors, personnel managers, anthropologists and political scientists and even men of cloth - are believed to have superior social knowledge based on training, study, reading, research - above all science. This knowledge and expertise is made available on request in professional situations. In contrast to this, "ordinary people" have very much to rely on their experience, what they have been taught and told, what they have read, etc. While professional knowledge may be conceived as more sophisticated, reflection makes it clear that it differs from lay knowledge in degree with regard to the criteria of reliable knowledge - such as being true, objective, proven, precise, etc. I would suggest that the main difference between the two variants of social knowledge is one of perceived status and authority; of a reputed and respected sophistication. The relative nature of this difference is well demonstrated when non-sociologists produce social analyses which leave sociologists green with envy.

It is true that a person's thinking, views and understanding of social relations and society correspond to his natural and formal socialisation - home, community, education and job. It is also true that the reception and diffusion of social knowledge from and among different spheres, are similarly stratified. When the quality of life rises, social stratification gives way to greater equality. What Mills calls the sociological imagination can then increasingly become a common denominator of social and cultural life. And not only because more social and societal information becomes available to the public. The social imagination develops as a way of thinking, a quality of mind, a sensibility of what human life, relations and humanity are all about. And those who are privileged with this sensibility should surely be able to understand the lack of it in other people, populations and strata.

One could conclude that professional and lay social knowledge do not differ all that much in substance. What differences there are, are related largely to different social conditions, qualities of life, social stratification. If professional social knowledge is more sophisticated, the theoretical interpretation appropriate in understanding the diffusion of scientific and professional social knowledge, would have to be a reception theory.

9. Rational thinking vis-à-vis empirical observation

Writing about the characteristics of scientific statements, Jeffrey Alexander (1982) draws a very simple but convincing little continuum. Placing metaphysics and observation at the two opposite ends, he makes the point that all scientific statements can be located on this continuum. Some will be rather more abstract, far removed from any observation by our senses; others a simple description of what has been observed. All statements contain both metaphysical and observational elements. Together with the assumption, which is now generally accepted, that there is no theoretically-neutral observation, Alexander's simple drawing demonstrates the inseparability of rational logic and empirical observation. Also in social knowledge these two are elemental constituents, apart from being two different criteria of validity of scientific statements.

It is significant that we often say: "Oh, I see", when we understand something, and "I *think* it is a woman" when we look at a person. The very natural confluence of sense perceptions and intellectual reflections, is, of course, the most basic assumption of all interpretation. But Interpretative Sociology, starting out with the personal perceptions and conceptions of particular individuals, often attains such metaphysical heights that this very human trait is forgotten. In such "pure" rational or logical constructions, ordinary social knowledge is little appreciated if not disregarded. Raymond Boudon (1988: 1) has essentially the same thing in mind when he says:

> Durkheim taught that sociology should break with common representations. To him, scientific sociology was to be developed against common sense.
> Today, with the retreat of positivism and rationalism, the dominant mood is oriented in the other direction: ordinary knowledge - another name for common sense and significantly a positive one - is now very much in. Its powerfulness and richness are celebrated and opposed to the rigidity and poverty of scientific representations.

Boudon's knowledge here is, I think, a sufficient reaction to Maxwell's wisdom.

10. Common sense and the social sciences

The reference to *common sense* brings to mind the often-heard comment that sociology establishes at great cost what everyone knows - presumably for free - and that, ultimately, its scientific knowledge is common sense. These two deflations of sociological work are not necessarily the same accusation. *Common* and *ordinary* are not simple words. Both can have the connotation of shared by many or most people, but they could also refer to: the *usual, everyday, unsophisticated* or *superficial*. Public opinion surveys and similar research usually report superficial reactions in descriptive terms but delve very deep in their statistics. Relevant here is the distinction between *descriptive information* and *explanatory* or *interpretative insight*. That "mere information" is at least a little explanatory, interpretative or theoretical, is now accepted by most social scientists. But still the distinction is of primary importance as two modes of knowledge and is essential in all analyses of social knowledge. When social scientific statements are disposed of as *common sense,* such statements would more often be theoretical than factual in nature. It is the explanations or interpretations of the social scientist which are being judged as "already known". This, of course, does not begin to analyse ordinary people's conceptions or their psychological and social criteria of acceptance/rejection of formal statements of social knowledge - "*Why* do you believe this?"

This is marsh country and one way to get onto firmer ground is to turn the question around: What is the influence of common sense on theories produced by the human sciences? And here we have professional help from a paper by Boudon: "Common sense and the human sciences" (1988).

Boudon argues that in many scientific discussions in the human sciences common sense plays a much more important role than is usually conceded. Boudon uses what he calls the "complex" notion of *common sense* which involves logic, theorising and explanation rather than fact, description and information. *Common sense* here refers to *principles* used in logical argument and rational enquiry. These are drawn from what can be called *commonsensical* linguistics, epistemology and logics. In ordinary, everyday life - and thinking - these principles are so familiar, so much accepted as universally valid that we are hardly conscious of them. Conceived as universally valid in everyday life, these principles are, however, seldom valid in scientific theories. Often their presence in scientific social knowledge goes unnoticed, allowing social theories more credibility than they deserve - at least in the eyes of ordinary people - who then, metaconsciously familiar with these principles, turn round and say: "These theories are just common sense"!

Examples of common sense logic, the validity of which is almost never questioned in everyday life, would be the following:

[1] Of two different explanations of a social phenomenon only *one* explanation can be true.

[2] A true theory represents a phenomenon as it *really* is and when dealing with past events, its tells us "*wie es eigentlich gewesen*".

[3] Everything has a cause and, often, more specifically, everything has *one* cause.

[4] There are explanations and *real* explanations. Most people have the idea that "explaining" means finding a real, *substantive* (if hidden) cause.

[5] The choice and preference for the real/more true/more meaningful explanation or theory, have a number of variations:

(a) A crime of theft for instance could be explained as a case of: personal and personality deviance; easy, tempting and conducive circumstances; a criminal subculture; prevailing economic conditions
(b) Genetic explanations concentrate on the development and succession of historical conditions and events; functional explanations give priority to social consequences of phenomena

(c) Impatience with explanations that propose a causal chain of factors or events, or the rejection of even one intermediate variable

Social scientists as well as lay people exercise their preferences among these possibilities. The preferences of social scientists would largely correlate with the chosen theoretical approach or model: psychological, sociological, functional, historical, etc. The lay public would most likely choose explanations which, for whatever reasons, seem to them more real and meaningful, perhaps least complicated.

[6] Boudon questions the cognitive status or quality of theories constructed by examples. (Referring to Kuhn's theory of science, he says that exemplification has never been considered a sound method of theorising.)

[7] Lastly, it cannot be denied that many social scientists and most ordinary people consider theories to be true, *because everybody believes them*".

These cognitive "principles", whether employed consciously or accepted metaconsciously, by social scientists or ordinary people, demonstrate the overlap, interplay and fusion, as well as the tensions and contradictions within social knowledge. It also demonstrates the very human fragility of social thinking and knowledge.

11. Technical language vis-à-vis ordinary speech

A news item from *The Cape Times* of 27.12.88 which the paper took over from a London source, is appropriate here:

> Scientists are given a stern warning in the current issue of Nature that unless they learn to write their papers more clearly and avoid bad grammar and laboratory jargon, there is a danger that their work will cease to have any impact.
>
> "That everyone can understand, should be the first of the New Year's resolutions", says the journal's editor, Mr John Maddox.
>
> Mr Maddox, who has had a continuous uphill struggle to teach his contributors to write intelligible English, says that scientific papers are different from detective stories, whose readers may enjoy suspense. The conclusion that the scientist has reached should appear at the beginning of his paper as well as at the end.
>
> Too many papers were awash with facts and numbers. The most serious enemy to clarity is the reverence for data. Writers are often so anxious that a date should be correct, but so utterly uncaring that what they say should be understood.

Need one comment or elaborate?

We can, of course, not do without technical terms. I would like to make two suggestions about them: (i) Sociological writing which both colleagues and neighbours find incomprehensible, more often suffers from illogical sentences, poorly constructed arguments and contradictory statements, rather than from those weird technical words. It is the logic of a well- written short story more than its simple language, which makes it an object of jealousy for sociologists. (ii) The words ordinary people use to articulate and implement their social knowledge should be respected for their connotation, tone and the situations in which they are used.

12. Conceptions vis-à-vis Concepts

Turning conceptions of social phenomena into Concepts in the process of theorising, is a tendency which Mills has warned against in *The sociological imagination*. I will briefly refer to three examples:

(i) Socialisation

Dennis H. Wrong's paper "The oversocialized conception of man" was written in 1961. It was then and is still an amber light warning against the overvaluation of the range and effectiveness - the decisive "functionality" - of a basic but differentiated process of acquiring normative patterns and learning what is socially expected of one. The process of *socialisation* has been over-theorised to the point where social conformity and deviance and societal order were all explained in terms of this process.

(ii) Values

The Parsonian-Kluckhohn concept of *values* is well-known. So too is Parsons's (1951: 12) original formal definition of the term:

> An element of a shared symbolic system which serves as a criterion or standard for selection among the alternatives of orientation which are intrinsically open in a situation.

Perhaps a little more realistic is the definition of N. Rescher (1969: 9):

> A value presents a slogan capable of providing for the rationalization of action by encapsulating a positive attitude towards a purportly beneficial state of affairs.

It was in 1937 that Parsons published *The structure of social action*. "After publication of *Structure*" says J.L. Spates (1983: 30), "the idea that values might be a controlling factor in social life took on a life of its own in American sociology."

From that time Parsonian sociology not only over-conceptualised *values* but over-theorised the particular conception to the extent that these orientations were postulated as the primary explanation for both social behaviour and societal order. For Parsons the fountain-head of all values is "ultimate reality", that transcendental space which ordinary people call heaven. It has remained a disappointment and an embarrassment for Parsonians that when he had to provide empirical evidence or mere examples of values in social behaviour, social and cultural systems, the categorisations just petered out.

It was Alain Touraine and C. Wright Mills who most eloquently commented on these theoretical excesses:

> The word (value) is dangerous because it designates a hodgepodge of cultural orientations, social rules, and ideology (Touraine, 1981: 79).
> ... political sociology and organization theory have shown us that decision systems - at least in our type of society - are not the application of principles but the result of transactions, a fact that recognizes a certain heterogeneity of the interests confronting one another.
> What then is a society's system of values? On the highest level it is the unity of a discourse held by the ruling classes or by the ruling political forces ... (Touraine, 1977: 5)

Already in 1959 Mills wrote:

> What Parsons and other grand theorists call "value-orientations" ... has mainly to do with master symbols of legitimation.
> Such symbols, however, do not form some autonomous realm within a society; their social relevance lies in their use to justify or to oppose the arrangement of power and the positions within this arrangement of the powerful.
> We may not merely assume that such set of values, or legitimations, must prevail lest a social structure comes apart, nor may we assume that a social structure must be made coherent or unified by any such "normative structure".
> Unless they justify institutions and motivate persons to enact institutional roles, "the values" of a society, however important in private milieux, are historically and sociologically irrelevant.
> We may not misuse the idea as the theory of social order or of the unity of society (Mills, 1959: 37-8).

Perhaps the South African anthropologist, Eleanor Preston-Whyte (in Joubert 1986), said it all so much simpler:

> In many real situations people have no choice so it matters little what their values are.

(iii) Culture

Culture is perhaps the idea that has been most overconceptualised and overtheorised in social thought and social science. This reification has, however, lately been turned around: contemporary anthropology and sociology are at present working on a different concept of culture. This is a conception which ordinary people should readily understand and recognize as part of their life-world. And one which social scientists will find theoretically more sensible.

In *South African keywords: The uses and abuses of political concepts* (1988) Robert Thornton refers to three successive conceptions of culture:

> ... the Romantic idea of culture is that it is the organic product of a people or nation. The idea owes much to the German poet Goethe and the philosopher Hegel (1988: 21.)
> In the twentieth century the Modernist idea of culture retained the notion that culture is uniquely associated with a single society or nation. But unlike the Romantic notion, it asserts that culture functions to maintain society (or nations), and that culture is historical and changes over time, often in relation to (or determined by) changes in "society" (or economy). Because culture - in the form of myths, political speeches, religious beliefs, ideologies, histories and tradition - is held to have a social function, the theory requires that each "culture" exists as a whole within a "society" (1988: 22).

What is now generally referred to as the *Postmodernist* concept of culture, Thornton chooses to define in terms of *resources:*

> Today, culture is best thought of as a resource, like other resources, such as energy, sunlight, air and food, it cannot belong exclusively to any particular individual or group of individuals. All groups and individuals must have access to at least some of these resources to survive (1988: 24).

Thornton also points out that although culture is an essential resource, not all people have equal access to all of culture. And, if we think of culture as a resource, we must include our own ideas about culture as part of the resource that culture provides.

The postmodernist idea of culture could be seen as a deconceptualised and detheorised notion of the term. I would contend that this makes the concept not only more theoretically sensible but also more humane, i.e., brings it nearer to our everyday experiences of the world or worlds we live in.

13. Culture and society

While not representing a "contraposition" in ordinary/scientific social knowledge comparable to other contrapositions, the distinction between "culture" and "society" confuse many ordinary people. To many social scientists, however, the distinction represents an indispensable theoretical insight.

In Modernist social theory *society* most generally refers to a large collectivity - like a nation or a people - whose field and frequency of interaction form a "whole" - an identifiable and separate "system". Culture refers to patterns of ideas - traditions which are transmitted from generation to generation - abstracted from their visable enactment. While "societies" are systems or patterns of *interaction*, cultures are systems or patterns of *beliefs*.

The distinction is, of course, an analytical one and generally considered indispensable in theorising. But as in the case of *culture*, the concept of *society* has become reified, especially in structural-functional sociology, to the point that one feels nostalgia for old-fashioned societal determinism. The overconceptualisation and extremist systemic notion of *society* are (unequally) indicated by the term *socio-cultural* - necessary to re-establish the "links" between society and culture, actions and beliefs - and, the amazing Parsons-Kroeber manifesto of 1958. Thirty years ago quarrels between sociologists and anthropologists over the concepts of *society, culture* and *social system* were such that their respective doyens put their signatures to a two page document of which the last paragraph read as follows:

> We therefore propose a truce to quarreling over whether culture is best understood from the perspective of society or society from that of culture. As in the famous case of heredity "versus" environment, it is no longer a question of how important each is, but of how each works and how they are interwoven with each other.

It is this contractual agreement which, eight years later, entitled Parsons (1966: 113) to declare: "I am a cultural determinist, rather than a social determinist".

Like (and in tandem with!) the concept of culture, our understanding of "society" is also in the process of being "postmodernised". It is still early days for this reconceptualisation but in a recent essay Zygmunt Bauman (1988) gives substantive outlines of the new conception of "society". Bauman's discussion of postmodernism in art, culture, world-view and sociology, draws upon the work of writers such as Brian McHale, George Steiner, Francois Lyotard, Harold Garfinkel, Hans Gadamer, Susan Heckman and Pierre Bourdieu. *Hermeneutics* is very much the name of the game.

Sketching the salient characteristics of the postmodern society, Bauman, predictably, allows "culture" and "social structure" to merge:

> Postmodernity is marked by a view of the human world as irreducibly and irrevocably pluralistic, split into a multitude of sovereign units and sites of authority ... (1988: 799)
> Our time is marked by the end to the hierarchic value structure and the rejection of the "binary cuts which represented the domination of the cultural over the natural code", like the cuts between the West and the rest, learned and untutored, upper and lower strata. The superiority of Western culture (cultures?) seems neither self-evident nor assured as a prospect (1988: 798).
> The old setting derived its solidity from the presence of mutually reinforcing, co-ordinated and overlapping agencies of integration.
> The totalizing impact of economic systemness, body politic, unified law, dominant value-cluster or ideology was tacitly assumed (indeed, it served as the very pre-condition of the possibility of discourse) and thus remained throughout the concealed, yet omnipotent guarantee of the authority of truth and meaning. The new, communal spaces ... are grounded in their activities only, and so expose the absence of synchronization between the truth-and-meaning oriented action and other dimensions of social existence (1988: 800).

Bauman's use of the terms *communal spaces*, *communities* and *discourses* is indicative of the postmodernist thesis: Increasingly, contemporary social experience or existence, and contemporary social knowledge or understanding of social reality, have become "regionalized" into different life- worlds and different discourses. Rather than experiencing the world as a "global village", ordinary people have maintained their life-worlds in particular villages if not suburbs. ("Visits between suburbs are rare, and no resident of the city has visited them all." - Bauman, 1988: 802) Social reality is not experienced or perceived in terms of universal parameters; social knowledge not developed in universalistic paradigms. Increasingly social life and knowledge are experienced, perceived and defined as *discourses*. The "significant others" are those people who are "worthwhile", with whom one can talk, converse in a common language, with whom one shares life-worlds rather than a society.

The image of postmodernist society which is gaining ground in both ordinary and scientific social knowledge, is well demonstrated by the work of Garfinkel. His methods of analysis expose "the endemic fragility and brittleness of social reality, its "merely" conversational and conventional groundings, its negotiability, perpetual use and irreparable underdetermination" (Bauman, 1988-804).

Modernist sociology believed its prime theoretical challenge to be the explanation of social order. It accepted as fact a pervasive and systemic order of societies and then turned it into a theoretical problem: What could possibly be the origin of such orderliness and how is it maintained? Formalistic conceptualisation and abstract theorising have left far behind the everyday experiences and conceptions of ordinary people. If social change has brought about "postmodernist" societies - characterised if not ordered by consumerism and television as Bauman suggests - modernist sociological conceptions of "society" may well be unrealistic, if not factually wrong. A rethink and reconsideration of sociological understanding of societies could lead to more realistic and meaningful social knowledge; realistic, meaningful and comprehensible also to many ordinary people. As Thornton (1988: 28) correctly says: "... 'a nation', 'a community', and 'a society' are categories of thought."

14. Knowledge applied vis-à-vis knowledge assimilated

The application or otherwise of social knowledge produced by the social sciences has generated a long and divergent debate. The discussion has mercifully progressed beyond the question whether social reform and change should be among the aims of social research and inquiry. Knowledge for the sake of knowledge is as uninteresting to debate as is *l'art pour l'art*.

In a recent essay Johann Mouton (1988) defines power as the ability to accomplish change. Bringing about change, he says, is not only a desirable aim, but an intrinsic dimension of all social science research. Social research and theorising constitute a social practice which has other social practices as objects of study, while at the same time being integrally linked to these other practices. The effect scientific social knowledge has on various social practices, is itself varied and of a wide range. Mouton (1988: 45-6) writes:

> Like all social practices (politics, language, religion, education), the practice of social science does not leave the people (whom it studies and investigates) unaffected. Social theorizing explains, interprets, predicts, analyzes, informs, demystifies, unravels and demythologizes, human behaviour and actions and in this way becomes part of parcel of those same actions and inevitably change existing practices and customs.

What Mouton refers to as "dominant epistemological traditions", have different "metaphors of power". Positivism translates *power* in the dictum *knowledge is power* as *therapy* or *medicine*. Positivistic knowledge lends itself naturally to the diagnosis of social ills, problems and deficiencies and, can with great confidence, prescribe cures, solutions and reforms. Interpretative Sociology has another ideal - that of *enlightenment*. Its aim is to give people a better understanding of themselves and greater insight into their life situations. This is Mills's *sociological imagination* - looking at both personal problems and social issues.

Lastly, Critical Theory, starting from the assumption that man is essentially in a state of alienation, dedicates itself to social knowledge that can help *emancipate* and liberate people from oppression and exploitation.

It should be clear that social scientific knowledge can be developed and applied in the achievement of various aims and ideals. Also, and more important, such knowledge has variegated effects - from providing simple factual information to demystification or holding ideologies in place.

It is particularly the range of possible effects that exposure to scientific knowledge has, that places a question mark behind the phrase *application of social knowledge*. I would like to suggest that scientific and ordinary social knowledge are taken note of - acknowledged - by individuals, turned over in the mind and then enacted or not, keeping the original content (as perceived or understood) intact or adapting it. "Application" of scientific findings, comparable to switching on electricity, is a very limited occurrence. Any suggestion that social thinking and action have to wait for appropriate scientific research for us to know what to do, is, of course, simply ridiculous.

The processes of diffusion, reception, assimilation and use of social knowledge - whether scientific or ordinary common sense - are much more natural and human than planners, reformers and professional changers would have us believe.

15. Societal reference and personal meaning

There is a conventional view that sociology is about society, social structures, social conditions and not about individuals, personal behaviour and beliefs. Thankfully, this convention has been left behind so that the margins of sociology, psychology, anthropology, history, even linguistics and other fields are no longer fenced-off from one another. *Social science, human sciences, social theory, social knowledge* - these terms now indicate a realisation that *personalities, social systems, cultures* - and I should have started with *bodies* (!) - are but convenient levels of observational and theoretical concentration when we seriously reflect on the ways in which human beings live and die.

There is one last contraposition in present sociology and social science which can be called: *Societal reference vis-à-vis personal meaning*. It is a contraposition which characterises and will remain inherent in sociology, social science and social knowledge - whether sophisticated or ordinary.

On a very practical level this contraposition is manifested in ordinary people's preference for social knowledge that stays closer to personal meaning than social knowledge in which the societal reference is dominant. This may be one reason why psychology is easier understood and more popular than sociology.

The old complaint about sociology has been that it is so "abstract". In ordinary thinking it takes about two seconds from *abstract* to *irrelevant*. Irrelevancy is the accusation with which we started this exercise.

To generate, assimilate and put to use social knowledge, it is necessary, and inevitable, to shuttle between levels of abstraction. To explore these levels is to explore our personal troubles and our social issues. If these are explored, travelled for the sake of discovery, with sociological imagination, we will, I believe, be moving forward towards a human sociology and a more humane society.

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6

Recent trends in educational methodology in South Africa: Are there any?

Wally Morrow

1. "Trends" in educational methodology

The ambiguity of the phrase "educational methodology" - meaning either "method of teaching" (which would be the more usual interpretation in the field of education) or "method of educational research" - is significant. This significance will be revealed later in this paper. In the mean time I shall proceed as if "educational methodology" meant "method of educational research" or "research methodology in that part of our social world which we call 'education'" (as opposed to what?)

A "trend" is a general direction which something tends to take, it is a general tendency in the movement, flow or change of some phenomenon. One might talk about the trend in the price of gold, or the political trend in South Africa, and what one would be doing is, on the basis of some observed pattern in the realm in question, to try to make something like a less than arbitrary forecast about how things are likely to change in the not-too distant future. Successfully to identify a "trend" in some realm is, in a sense, to gain a better understanding of it. To have identified the trends in, say, political life in South Africa is to have provided oneself with some intellectual tools in terms of which one can better understand particular events as they occur. And once one has successfully identified trends in some realm one is in a better position to co-ordinate one's actions with what is likely to happen, to orient oneself in an otherwise confusing world. Someone might also be interested in trends so as to avoid being caught in last-year's fashions.

In the first part of this paper I shall argue that the project of discovering "trends" in educational methodology in South Africa faces overwhelming odds, and then I shall argue that, in any case, there are no trends in educational methodology, particularly in South Africa. I shall specify five forms of

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ignorance and then claim that anyone who thinks that we *could* find out what the trends of educational methodology in South Africa are, or who thinks that there *are* trends in educational methodology in South Africa, is suffering from some combination of these forms of ignorance.

(a) Someone who thought that we could find out what the trends in educational methodology in South Africa are might simply not know how crowded the field of educational research is. I don't know how one might compare the sheer numbers of people who see themselves, or are seen as, doing educational research in South Africa with the numbers of people involved in, say, agricultural, economic, biological or historical research, nevertheless, and this is not merely a question of which labels one uses, the arena of educational research in South Africa is swarming with competitors, and for fairly obvious reasons.

Schooling has been the site of some of the most dramatic and bitter political struggles over the past few decades, and this turmoil has attracted many people who see themselves as being able to contribute by means of some kind of "educational research" to either the delegitimizing rejection or the band-aid maintenance of the schooling system. Then there is a more anodyne type of dissatisfaction with the schooling system, mostly amongst "parents" or "employers", for not delivering the goods (whatever they may be.) Such dissatisfaction has made its contribution to the flood of educational research. Then there is the fact that people from diverse academic backgrounds get involved in "educational research" possibly because they see it as not as demanding as other types of research, possibly because they like the idea of doing "useful" research. Finally, the teaching profession is very large, and, for the purpose of clawing their way up the hierarchies of the bureaucracies of schooling, many teachers do higher degrees in education and, thus, get drawn into doing "educational research".

In such a crowded field we are unlikely to be able to identify "trends".

(b) Furthermore, someone who thought that we could find out what is going on in the field of educational research might not acknowledge the great difficulties in trying to get information about what is going on in this field. What could be one's sources if one set out to try to provide an even passably comprehensive broad overview of the crowded arena of current educational research in South Africa? One source might be education journals. But in South Africa there is no education journal that can lay claim to represent even a reasonable sampling of the wide range of educational research; and, in any case, there is a great deal of educational research published in other journals or which never reaches the pages of academic journals. The most readily-available listing of research topics is, obviously, various HSRC documents. But this source will fail to reflect extensive bodies of other educational research which, in the longer term, are likely to be of much greater importance.

To gain some impression of the extent of such other research we might consider the divergence between items listed in HSRC documents, and those listed in Kallaway's bibliography.[1] We might think, also, of research being undertaken by those (many, but not all, of them ex-patriate South Africans) located at universities and research institutes outside of South Africa, or pursued by those who regard the HSRC as so obviously locked into the ideological apparatuses of the Capitalist-Apartheid State as to be an absolutely unacceptable source of funding. But even these considerations do not yet bring into view the great volume of educational research which will not be reflected in HSRC listings not for explicitly political reasons but because it is not for degree purposes or does not bid for HSRC funding.

We can add here that the general climate of suspicion, censorship and the restriction on the flow of information in this country also has an effect on the flow of information about, and thus the conduct of, educational research. There are many, including many who are in the business of educational research who, justifiably in some cases, are anxious to keep a low profile to avoid the attention and possible wrath of the authorities or their surrogates. But such a situation distorts and restricts the field of educational research, and provides plenty of space for illusions about the quality of the research being done. On the other side we can add that it is always easier to be a spectator shouting rudely from the stands than to be a player out on the field.

But if there are these difficulties in finding out what is going on in the field of educational research, then the project of identifying "trends in educational methodology" looks unviable. But, in any case, does this way of talking make sense? Are there, *any* trends in educational research methodology in South Africa? Someone who thinks there are, probably suffers from one of the following three kinds of ignorance:

(c) They may be unaware of the perennial problem of how one might specify the scope or identity of educational research. The conventional opinion here inclines to the view that education and schools are intimately connected, maybe the same. If we hold this conventional opinion then we might think that we have a fairly clear boundary to the scope of educational research. But this is not so, schools are institutions located in historical contexts constituted by political, cultural, economic, conceptual and other forces, so even on the conventional opinion educational research includes research in these other spheres and does not have a clear boundary. Furthermore, the conventional opinion is challenged as soon as we begin to think of something which we might call "the educational process". There are those who say that "education" is a "life-long process", and then they think of preprimary schools, and tertiary institutions, or, because they don't want to confine "educational research" to what goes on in institutions, they think of "adult education" or "non-formal" education (and they imagine that they have made a bold discovery which deserves high levels of research funding.)

But we can't stop even at this point, once the school doors are opened. The "educational process" also includes upbringing, and the many kinds of influence that there are on people, such as the mass media, the various kinds of organizations in which people get involved, or the informal and casual influence people have on each other in daily life etc. And as human beings are biological organisms of a particular type, research into their growth and development is clearly relevant to a consideration of the "educational process". And tangled into all of this there are considerations about values, mind and knowledge, the kinds of issues traditionally considered in the philosophical disciplines of moral philosophy, philosophy of mind and epistemology, etc. This brief catalogue of issues is an indication of the kind of problem we face in trying to specify the scope and identity of educational research. We can note that in such an unspecifiable research field it makes little sense to try to identify "trends".

We can add to this tale that the scope and identity of educational research, ever since people began to think that there could be something called "educational research", have always been vigorously contested. Attempts to short circuit this contestation by clearly demarcating the boundaries of the field have seldom held sway for long, and usually their influence on educational research has been deleterious. There have been those in this country who have tried to shut down the dispute by claiming that "education is an autonomous science", but this attempt, like the others mentioned above, has damagingly limited an understanding of education and turns out to be little but a defensive wall around a private playground.

(d) They might be under the false impression that there is something which could be called an "educational research community" in South Africa. In South Africa there is no association or journal which can justifiably claim the support or allegiance of any substantial fraction of the swarms of researchers in the field of education. Faculties of Education at universities, apart from having little by way of serious academic contact with each other, are, typically, chronically locked into factional and destructive internal power struggles for control over research agendas, with little by way of critical discussion about the nature, purposes and objects of educational research.

This lack of a coherent research community in the field of education has at least three important consequences. First, as there is almost no generative dispute or debate amongst, and between, those many people who see themselves as doing educational research; the field consists of an ongoing babel of isolated and stuttering discourses bidding desperately or stridently to maintain or gain the high ground of influence over the politics, institutions and practices of education. Second, the reference groups which adjudicate between acceptable or unacceptable research, between promising and less promising research projects or programmes, are suspected, often rightly, of serving sectional or factional interests which have nothing to do with education, rather than with the uses to which "education" can be put. Third, without a coherent research community, the field of educational research tends to become merely a platform for demagogues isolated from critique by their status in a bureaucracy, their institutional location or, their popular support.

Where there is no "research community", to talk of "trends in educational methodology" can, at best, be a cudgel with which to batter one's opponents or a polemical attempt to define the field to suit one's own interests, or those of one's "group".

(e) They may be unaware of the radical disarray in the field of educational research. This disarray consists in rival and incompatible conceptions of the nature of knowledge about education - it might be described as epistemological disarray. The lack of an "educational research community" in South Africa and the epistemological disarray I am talking about now are two sides of the same coin. There might be a question about which of the two - the lack of a research community or the epistemological disarray - is explanatorily more fundamental, about which is a symptom of which, but it is not necessary to my present argument to try to answer this question.

The disarray in the field of educational research is quite different from the kind of challenging diversity of opinion which, in some research fields, can provide the spur to greater rigour and prompt the search for more fundamental or imaginative solutions. The disarray in the field of educational research is charaterized by fundamental lack of agreement about every aspect of such research. There is dramatic lack of agreement about what "research" is in this field, about what its uses and purposes are, and even about what the research object is. Due lack of agreement at this level it is frequently difficult to know what is under discussion when people talk about "educational research" and there is no way that we could talk of "educational methodology" as if we had something identifiable in view. To talk of "trends" in "educational methodology" merely begs this question.

Some who might feel inclined to challenge the claim that there is radical disarray in educational research might be blinkered by the parochialism of a discipline. The disciplines involved here might be either those of the academy or those of political struggle - to the extent that these two kinds of discipline are distinguishible. People so blinkered might regard some patch of educational research as in reasonably good working order, indeed they might regard such a patch as the whole of the field. Within such a domesticated patch it might be possible to "identify some important methodological issues", or some "recent trends in educational methodology", and this might provide the substance of a paper such as the present one. To the extent that they acknowledged that their domesticated patch does not comprise the whole field of educational research, they are likely to see the remainder of the field as occupied by a fractious rabble stridently engaging in a chaotic range of activities, often self-described as "research", which can hardly bear this pregnant title, and which, on a little probing, turn out to be sham, mere subterfuges for other purposes and agendas.

But this manoeuvre, strongly reminiscent of the indefensible claim that education is "an autonomous science", is so obviously phoney as not to pose a serious challenge to the claim that the field of educational research is in disarray. And of course, if educational research is in disarray then we can make no sense of talking about "trends in educational methodology".

But if we know all this - if we know that in South Africa the field of educational research is crowded, that there are great difficulties in gaining information about what is going on in this field, that there are enormous difficulties in specifying the scope of educational research, that there is no "research community", and that the field is in radical disarray - then we can conclude that we can give no substance to a project of identifying trends in educational methodology. This is so for the obvious reason that there can be no "trends" in a chaotic range of disparate phenomena; but any even reasonably informed view of research in the field of education in South Africa must conclude that it is chaotic and disparate; thus a reasonably informed view of educational research in South Africa must conclude that there are no trends in educational methodology in South Africa. And it will make no difference if we talk about "recent" or "current" or "remote" or "future" or "past" or any other trends.

And this, really, is my response to the topic of this paper. But let me make a few further comments before I close down.

2. "Official" educational research in South Africa

An informal survey [2] of (a) the titles of articles published in the *South African Journal of Education* [3] from 1986 to 1988, (b) Applications in 1988 to the HSRC for educational research funding, and (c) Applications in 1988 for D Ed/PhD (in the field of education) bursaries, yielded the following information.

In all three sources the most popular topics (a 13%, b 29% and c 25%) were those which fell into the category of the teaching of particular school subjects- with the majority of such topics being about maths and science teaching. Typical examples of topics in this category are as follows:

- Towards an understanding of two-digit numbers - a theoretical perspective on learning contexts.

- Natuurwetenskaplike geletterdheid deur informele onderwys.
- Shakespeare and schools in Southern Africa - teaching and examining Shakespeare drama at first and second language levels.
- Rekenaar simulasies vir opleiding en navorsing in Biochemie.
- Investigation into the learning-teaching dialectic in mathematics problem solving at tertiary level.
- 'n Meerdimensinele model vir die ontwerp en ontleding van Aardrykskundevraestelle op senior sekondêre skoolvak.

The only other category amongst the five most popular in all three sources (a 11%, b 7% and c 14% - in the case of both a and c this was also the second most popular category) was that to do with students/pupils - their self-concepts, characteristics, expectations, study-habits, etc. Typical examples of topics in this category are as follows:

- Gebrekkige konsepvorming van leerlinge wat deur 'n vreemde medium onderrig ontvang.
- Die verband tussen uitvalle ten opsigte van die items van 'n skoolgereedheidstoets and latere leerprobleme.
- An aid for investigating the relational image of the primary school child.
- Die voorkoms van diefstal by primêre skoolleerlinge.
- Student selection using a model which could predict success in first-year biological studies at university.
- Study habits as a dimension of personal competence.

Of course, a classification of topics according to content (which is becoming more prevalent as the computer-listing of research topics encourages the increasing use of "keywords") tells us very little. Apart from the problem of into which of any given set of categories some topic is to be classified (this is a heavily interpretive exercise which can readily become merely idiosyncratic and arbitrary), there is the problem of which system of categories is being used, different categories cut the cake very differently. This is a problem at the conceptual or theoretical level, which an empiricist theory of language simply cannot comprehend. To illustrate this problem, I shall discuss a Research Newsletter issued by the HSRC in October 1988 *Education - Priorities for Research.*[4]

This Research letter provides a brief report on the on-going work of the Main Committee of the HRSC Education Research Programme. At the beginning of 1988 the Committee decided to re-identify education research priorities. It was decided that "a Delphi type technique consisting of two phases" would be used.

education at universities, teachers' associations and certain private sector bodies that were closely involved with education in the RSA. Questionnaires containing a provisional list of ten priorities as identified by the Main Committee were sent to these "interest groups", and the respondents were requested to supplement the list with at least a further five priorities, and, on the resulting list of 15 "priorities", to rate the five most important, in order of importance. There were 91 respondents, and, in total, they added a further 91 topics to the original 10.

The compiler of the Research letter notes, significantly, that "In virtue of the divergent nature of the priorities presented, it was decided to divide the priorities into 11 categories. *This division is somewhat artificial as the categories overlap and interact closely*" (My emphasis.) After weighting the response patterns etc, it was "evident" that the ten most important priorities in educational research in South Africa were the following:

1 Career education
2 The design of primary and secondary school curricula
3 Accreditation
4 The teaching of physical science and Mathematics
5 Bridging phases or colleges
6 Views and expectations of education
7 General education affairs against own affairs
8 The teaching of languages
9 The curriculum of tertiary education institutions
10 Teaching methods in primary and secondary schools.

We can note that this is a very mixed bag of items; they are mixed in respect to both the levels at which they operate and the kinds of concerns which motivate them, there seems to be no unifying principle of classification.The various items on this list of research priorities pull in various directions; the criteria or motivations for inclusion on the list look radically disparate. The various "priorities" on this list cut the cake of research in divergent directions and dimensions, and express a confusion of views about the objects and purposes of educational research.

Leaving aside such problems, which I shall discuss in what follows, and reading this list at a literal, and superficial, level, we can note the following. Given the radical turmoil, the prevalence of structural and other forms of violence, and high levels of political and bureaucratic chaos in the schooling system in this country, this is an amazing list of educational research priorities in South Africa at this time. In the light of the chronic disarray of schooling in South Africa, and the symptomatology which has emerged out of widely-supported oppositional and resistance movements, the selection of Career education (the compiler of the letter notes that "this priority received the greatest support by far"!) as the highest educational research priority, would strike an impartial observer as simply bizarre. Such a judgement would be unaffected by the following gloss on what is meant by "Career education":

> ... career education should not be regarded as vocational training but as an integral part of general education and as a matter that should be presented as part of every subject rather than as a separate subject.

Furthermore, even the current regime, struggling to maintain some sort of control over the anarchic schooling system, can hardly view with much enthusiasm this list of ten priorities for educational research produced by supposed "experts" about education in this country.

The second phase of the investigation took the form of a one-day seminar, on 23 May 1988, to which representatives of all the institutions involved in Phase 1 were invited. Those attending the seminar were presented with the findings of the first phase, divided into five groups, and asked to "discuss the priorities so as to identify, refine and express further views on the most important of them." In more detail, each group was asked to:

(a) Identify the category that needs to be researched most urgently;
(b) identify the themes that should be researched urgently within the identified category; and
(c) define each theme in respect to what should be researched.

We can note that this agenda can be read as an appeal to the groups to achieve some measure of conceptual clarity about the category identified as requiring the most urgent research.

The five groups each identified the following as the main priority in educational research in South Africa:

Group 1	Educational expectations
Group 2	Career education
Group 3	The gap between existing opportunities and existing expectations.
Group 4	Primary and secondary education, particularly the need for a more applicable (toepaslike) curriculum.
Group 5	Education policy affairs.

In the Research letter each of these "priorities" is spelt out briefly by means of a few further points (which, to judge from the mixed styles, were written by the groups themselves.) One might have expected that we would move here towards some clarity. But, far from illuminating how the relevant "priority" is to be understood, the further points, in all five cases, merely obscure and confuse what could be meant by what is chosen as the most urgent priority. As an example, I shall quote in full, the report from Group 1:

> Group 1
>
> This group felt that *educational expectations* overreached all the priorities and consequently singled out this category as the most important. The following themes were identified within this category:
>
> (a) Ways in which education can be presented more effectively. The relations between education and the world of work should be investigated as well as the link between education and the country's economy to ensure optimal utilization of opportunities, resources and facilities.
>
> (b) The most effective financing of education. In this regard the most effective ways of spending education funds and the way in which the community ought to be involved in education financing should be determined.
>
> (c) Supplying sufficient qualified manpower. Issues that should be investigated here include models that can be used to raise the quality of the initial training and of in-service training and the identification of survival skills that can be taught to pupils.

So, having identified "Educational expectations" as the category that needs to be researched most urgently, Group 1 identifies the following three themes within this category; ways in which education can be presented more effectively, the most effective financing of education, and supplying sufficient qualified manpower. It is extremely difficult to understand how these "themes" connect with "educational expectations" or to resist the impression that we have to do here with the merely idiosyncratic and arbitrary. Maybe the phrase "educational expectations" is a mysterious code for something else altogether?

The kind of conceptual chaos evident at this level is carried through to the further detail of the first theme. Having formulated the theme of "Ways in which education can be presented more effectively", the Group goes on to explain that this means that the "relations between education and the world of work" and the "link between education and the country's economy to ensure optimal utilization of opportunities, resources and facilities" should be investigated. The research programme is as follows: investigate the relations between education and the world of work and the link between education and the economy, and this will constitute research into ways in which education can be presented more effectively, and this in turn will be a contribution to research about "educational expectations"! This is Humpty Dumpty at his most bizarre.

In a Conclusion the compiler of the Research letter tries valiantly to sum up the investigation. He evidently wants to be as fair as possible to the various contributors to the exercise, but he clearly has enormous problems about how to pull it all together. The priorities chosen in the first phase appear to be radically divergent, this divergence is compounded in the second phase, and the co-ordination of the results of the two phases looks impossible. The following are some extracts from the Conclusion:

> Although there was a divergence in the feedback of the groups - expectedly - certain trends could be identified. It is clear that there was general concern about the quality and relevance of primary and secondary education and curricula; that a certain crisis orientation could be discerned; that it was realized that education could not rely on a higher percentage of state funds; that educational standards should be maintained; equal quality education ensured and educational facilities utilized optimally.

How these "trends" might be extracted from the investigation, certainly from the information given in the Research letter, is utterly mysterious. What has happened, for instance, to the "priority which received the greatest support by far", Career education; or to Group 5's suggestion of Education Policy Affairs, as the most urgent research priority? These items seem quietly to have been slipped off the agenda. The Conclusion continues:

> Against this background it is important to realize that irrespective of the priority singled out at the most important, *actions to solve the problems of primary and secondary education* are extremely necessary.

This looks like a desperate move (my sympathies are with the compiler). Are we now to understand that the complicated exercise to determine empirically what some individuals and bodies in South Africa see as the priorities for research in education, including the entirely admirable (if expensive) project of bringing the contributors face-to-face to discuss the matter and come to some agreement, are to be ignored in favour of a formula which is at such a high level of generality that it is utterly useless as a research agenda? How on earth is the formula "actions to solve the problems of primary and secondary education" supposed to guide a *research* programme? The compiler's problem was to try to find a formula which was simultaneously sufficiently general to give the impression that "consensus" had been reached in the two phases of the investigation, and sufficiently specific to guide a research programme. Faced with an insoluble problem the compiler opts for a formula which at least appears to express consensus.

But the confusions continue; once we are into a spiral of conceptual anarchy literally anything goes, and because everything is unexpected nothing is a surprise. What we read next is that:

> On the whole it appears that the central problem area requiring research is that of *relevant education* ...

The only way this conclusion could "appear" is as a ghost appears, inexplicably, frighteningly, and vaguely. What reasoning could have given rise to this conclusion? On the other hand, maybe we are now into the area of "decisions" as opposed to "reasoning"; when language goes on holiday "reasoning" loses its grip. But, however we got here, we have moved into a wilderness.

No problems are solved in education by talking about "relevance". We cannot determine the relevance of anything prior to a determination of to what it is supposed to be relevant - the claim that something is "relevant" is empty until we know how the claim continues. A school curriculum, for instance, can't be *simply* relevant (or irrelevant), but it might be relevant to maintaining the current dispensations of power, or it might be relevant to preparing the ground for a non-racial, democratic, post-Apartheid South Africa, or it might be relevant to the more efficient production of a docile workforce, or it might be relevant or irrelevant to all sorts of other things as well.

Driven by a self-description of their research as "value-free", investigators in the field of education, as in other fields in the social sciences, reach for concepts which they think offer scope for "neutral" empirical investigations. But there is no empirical investigation which could fix the meaning of "relevant education", or answer the question of which education is "relevant". Far from providing an escape from hard political debate, the phrase "relevant education" lands one right in the middle of it, and the words "scientific", "objectivity", "neutrality" and the like become merely blunt weapons of political struggle.

The conclusion ends by bringing together "primary and secondary education" and "relevant education" into a single formula, and informing us how some of our dwindling financial resources will be used in 1989 to cope with the problems of education at the level of educational research:

> During the Main Committee meeting held on 7 June 1988 it was decided to tackle the relevance of primary and secondary education as a priority research theme in 1989. A work group was appointed to prepare a research proposal and submit it to the Main Committee. This proposal will later be submitted to the research community to give those interested the opportunity of participating in the research.

We may wonder who comprises the "research community" so blandly mentioned here, but let's not concern ourselves with that at present. The effort to determine educational research priorities lands up with what can, with the best will in the world, only be regarded as a most unhelpful and unexciting answer: "the relevance of primary and secondary education". Furthermore, recalling, again, the historical situation we are in, this answer is either irrelevant or absurd. But how did this happen?

There is a number of possibilities here, but I shall fleetingly consider only one of them.

One is to say that those involved in the investigation, especially, probably, those who designed it and compiled the Research letter, are dishonest. On this reading the investigation is nothing more than a camouflage for other purposes. The gap between the "data" and the "conclusions" - on which I have focussed attention in my critical comments above - can easily be occupied by political agendas; and the vagueness of the "conclusion", far from being a deficiency, as I have claimed above, is a virtue, as it leaves space for the free play of various kinds of prejudice, bias and misdirection. I don't have much sympathy for this kind of explanation, but I don't now want to pause to explain why not.

Another kind of explanation would use the causal language of Marxism. Such an explanation would, no doubt, rely heavily on the phrase "the contradictions of the Capitalist/Apartheid State". It would, probably, consist in showing the ways in which the "contradictions" between the forces and relations of production, which are endemic in a Capitalist economy, which lead to crises in Capitalist States, and which are symptoms of the inevitable demise of Capitalism, are reflected at the level of ideology. The field of educational research is ideologically saturated, it is itself a site of ideological struggle, and there is thus no mystery about why an attempt to achieve agreement on a list of educational research priorities should come to such an unsatisfactory conclusion.

We can note that this kind of explanation, unlike the previous, does not impute evil motives to individuals or groups, it explains the difficulties they run into in terms of background causes over which they have no control. Their practices are self-defeating, but not because they are evil or stupid but because they are trapped in an historical situation from which there is no escape this side of revolution. I have more sympathy for this kind of explanation than for the previous one, although I think that the language of Marxist theory is very widely misused (I think particularly here of that much abused and overused technical term, "contradiction"), and to the detriment of the real challenge which Marxist theory poses to traditional and well-entrenched thinking about education and educational research. Nevertheless, in spite of what some people seem to assume, Marxist Theory is not nearly as uncontested as it seemed some years ago, and I don't have the time, now, to get embroiled in the difficulties involved.

The third kind of explanation, the one I shall explore briefly in a final section, might, in an extended sense, be called "epistemological". It touches on the question about the role of language in the constitution of the social world, and especially, in this case, the role of language in relation to the practice of educational research, and what this implies for "educational methodology". What I shall claim is that those who conducted the HSRC investigation into "educational research priorities" are trapped in an epistemology that defeats their purposes.

The Research letter is very slight, not, as such, a very significant or important document. But the deficiencies so crudely revealed in this little document run like an insidious cancer through the bloated body of educational research in South Africa; at least this is my implicit suggestion in what follows. The epistemological convictions that underpin the HSRC investigation are shared by many across the political spectrum of educational research in South Africa.

3. Problem and method pass each other by

The question which the HSRC investigation attempts to answer is indeed an important question, and it will remain so irrespective of who holds the political whiphand. The question is: Given not unlimited resources for educational research, what are the priorities? Which educational research should we foster, and support?

The method used by the HSRC to answer this question has virtues. It sets its face against the assumption that there is some established political or academic doctrine, party or leader who knows the truth and can impeccably decide on educational research priorities. The method signals strong opposition to answering this question by mere decision or political fiat. It sets out to draw "interested parties" into participation in the investigation. "Interested parties" are sent a list of ten research priorities, if only to provide a kind of concrete example of the logical form of a statement of research priorities. The participants are invited to add a further five priorities and then to rate five of the priorities in order of importance. We can note that on this format it is possible for a participant to choose none of the priorities offered on the distributed list, and to rate as the most important only the priorities he/she has added. Then, as a second phase, participants are invited to come together to discuss the results of the first phase, and reach some agreement. The explicit determination not to impose a pre-decided outcome on the investigation is evident at each stage. If one is interested in democracy then one could say that the whole method here looks admirably democratic. What, then, goes wrong?

Well, one might begin where most critics end. Who are the "interested parties" participating in this game? Let's consider this.

Motivated by the politics of suspicion, one might say that the fatal defect in the method used in the HSRC investigation lies in the selection of the 120 "interested parties". Certainly a similar investigation conducted by some oppositional group would have had a very different list of "interested parties", and would, as a result, probably have arrived at a very different set of educational research priorities.

The description given in the Research letter of the 120 parties invited to participate might fuel one's suspicions; it sounds very much like a catalogue of those most heavily committed to the defence of the current hegemony, those with the strongest interest in contributing to the "passive revolution":

> In the first phase 120 individuals, bodies and organizations were identified, including departments of education, teacher training colleges, faculties of education at universities, teachers' associations and certain private sector bodies that were closely involved with education in the RSA.

However, the looseness of this description, and the word "including", leave open the door to the possibility that groups, such as, for example, SACHED, COSAS, WECTU and NEUSA participated in the investigation. But, even if they did not, maybe the Committee tried, unsuccessfully, to invite them, and organizations such as COSATU, the NECC, the UDF, and the ANC. Or maybe they didn't even try, under the reasonable conviction that their overtures would simply have been rejected out of hand. Or, much more likely,they didn't try but on the ground that the determination of research priorities is, after all, a "scientific" question, demanding a "scientific" answer. In the case of such questions the answer needs to be sought amongst those who know what they are talking about in the field in question, in this case, such organizations as departments of education, teachers' associations, and the like. It would be simply inappropriate to ask for the opinions of every Tom, Dick or Harry.

If the Committee did reason in this way, there are two obvious objections. One is that at least some of the organizations I have mentioned above, such as, for instance, SACHED and the NECC have at least as much claim as some of those listed in the Research letter, to be regarded as knowing what they are talking about in the field of education. The other is that decisions about research priorities, except maybe in some rare cases in the natural sciences, are always political decisions, they bear on interests other than those of the researchers. This consideration is overwhelming in a field such as education which is saturated with politics - indeed education is simply one of the aspects of the political life of a society. The question: Which educational research

117

should we foster and support? is transparently political. Until we acknowledge this, no satisfactory answer can be given to it, problem and method will pass each other by.

In spite of my reference to "interests" above, I do not hold the view that there is some general criterion for distinguishing between political and other kinds of questions, nevertheless I do think that some questions are more clearly political than others. And political questions need political answers; the methods of answering them need to match the questions. If one is a democrat then there are particular procedures appropriate to the solution of political problems. A key principle of these procedures is that persons have the right to participate in the making of decisions about matters which affect them. We live in a culture in which "research" is seen as the rational response to any problem, and the status of "research" in our culture is a symptom of this. Most problems in education, especially in a morally distorted society, are political problems, and a lot of educational "research", on the left and the right, is directed towards the solution of political problems. Much of this "research" is ineffectual, it misses its mark - problem and method pass each other by. However, I don't in this paper want to get involved in argument about this issue or to try to propose a general criterion for distinguishing between "research" and the procedures of political decision-making, except to say that both have their standards of rationality.

If we ask for a roll call of those people, organizations, parties and groups who have a legitimate interest in the determination of "educational research priorities" in South Africa, we would certainly have seriously to consider not only those referred to in the HSRC description but also those organizations mentioned above, as well as many other organizations, many of which have been restricted or banned, and a lot of people either in the current regime's black books or actually in detention or under restriction orders of various kinds. Organizations and people who are in this situation precisely because of the views they hold about education.

Had the range of "interested parties" been extended in this way we could expect the results to have been very different from what they are. It seems highly unlikely for, example that, "Career education" would have received "the greatest support by far" in the first phase, or that "General education affairs against own affairs" would have been rated amongst the top ten. The second phase would certainly have been much more complicated, and even more difficult to sum up in a brief page or two; at least we can guess that, through bitter experience, these other groups would have been much more vividly aware of the political dimensions of the game they were in, much more sensitive to relations between power and truth.

Nevertheless, the main difference would have been a difference in *content*, a difference in the number and variety of ingredients in the pot; how one might prepare (I deliberately avoid the word "cook") those ingredients for consumption would still remain a critical problem. In other words, the problem of how to get a research programme out of the investigation, would be at least as, and probably much more, difficult than the problem valiantly struggled with by the compiler of the HSRC Research letter. The question of who is participating in the game is important, and this is the primary question in our current situation. But there is also an important question about the nature of the game being played, and this is a question which will remain even after we have sorted out who the players are. I shall conclude with a few brief comments about a complex range of issues.

The crippling, one might even say, fatal, problem in the game being played is that of how to co-ordinate the divergent responses of the participants. We can note that this problem is also endemic in democratic theory. The problem arises out of an assumption that the responses of the participants are "data", similar in epistemological status to the data we might gather in an empirical investigation of some aspect of the natural world. This assumption is revealed in the manner in which the responses are treated in order to co-ordinate them. They are treated as if they already had fixed meanings, as if each participant is contributing an already complete answer to the question. But this assumption is founded on a misconstrual of what (human) responses are, it misconstrues the epistemological role and status of responses in the investigation.

Those responses are themselves already in language and they express a particular understanding of the world, they are like contributions to a discussion or a conversation, and need to be treated as such. The contributions to a discussion are not "data", they are typically interim and tentative attempts to achieve some clarity, and maybe agreement, about what is at stake, and what the nature of the problem is. The problem itself becomes a locus of meaning and significance. There is a crucial difference between consultation and participation, and agreement reached as a result of participatory discussion is unlikely to reflect, unmodified, any of the initial contributions.

The game needs to be based on the acknowledgement of the extent to which knowledge and the social world are constituted in language, and that it itself is contributing to that constitution. This is a shift away from the view that language is simply a means for the communication of thoughts and attitudes which are essentially independent of language. To understand this is to understand the fundamental importance of the distinction between consultation and participation. Once (human) responses are seen, and treated, as contributions to a discussion, rather than as data, then we can understand the ways in which the practice of research serves to sustain or undermine, to create or destroy, webs of meaning, significance and practices. The research process itself can give rise to an understanding of the research problem which was not previously available, and it can generate the collective discourse which is a necessary condition for any other agreement. A shift of this order in one's understanding of the game being played brings in its train a more clear-sighted perception of the role of power and authority in research, and a more adequate conception of what could be meant by "fairness", "impartiality" and "consensus" in this context.

Were the game changed in this way, research in the field of education would be a very different activity, and a different range of "methodological issues" would emerge. One thought I have is that "educational research" would cease to be the playground of some spectatorial experts trying, typically fruitlessly, to discover nomological statements, if not yet "scientific theories", to hand out to "practitioners" for application. Some aspects of educational research would draw much closer to the critical and reflective activities of practitioners striving to gain a more clairvoyant understanding of their practices and, in the process, discovering how to overcome their political or other shortcomings. In this situation a clear distinction between, on the one hand, "methods of maintaining or transforming educational institutions" and "methods of teaching" and, on the other hand, "methods of educational research" would tend to break down; the phrase "educational methodology" would lose some of its ambiguity. Such an outcome would not suit the bureaucracies of university administration as (critically reflective) institutional change and (critically reflective) teaching would have to be reclassified as forms of educational research, but how would we count such activities for the purposes of awarding subsidy or the confirmation or promotion of university teachers?

I have spoken of the struggle by the compiler of the HSRC Research letter to draw out a fair and reasoned conclusion from the investigation: his problem is a problem at the level of the theory of meaning which guides the game. This theory of meaning is widely prevalent in South Africa, runs like a poison through a great deal of so-called educational research on the right and the left, and explains why problem and method so frequently pass each other by in the field of educational research.

In my discussion in the first section of this paper about why I think there are no trends in educational methodology, especially in South Africa, I assumed a conventional understanding of the word "trends". In such an understanding a statement of "trends" is something like a statement of fact about some range of phenomena. There is, as we can now see, another way of understanding it. Here a statement about "trends" is not a statement about some independent range of phenomena, rather it is an attempt to establish a particular order in an otherwise chaotic and unintelligible social flux. And this is a political task. On this understanding of what it is that one is doing in making a statement about trends, I have missed my cue.

Notes

1. P. Kallaway, J. Kallaway and D. Sheward *A Bibliography of Education for Black South Africans,* Cape Town, School of Education, UCT, 1986.

2. The table below provides an indication of the five most popular topics in three sources: (a) *The South African Journal of Education* 1986 to 1988; (b) Applications in 1988 to the HSRC for research funding; (c) Applications in 1988 for D Ed/PhD (in the field of education) bursaries. I express the results in percentages of the relevant sample each time. Where the percentage is in parentheses this indicates that this topic was not amongst the top five in that sample.

	(a)	(b)	(c)
Teaching particular school subjects (maths and science were predominant)	*13*	*29*	*25*
Students/pupils (characteristics, expectations, study habits, selection)	*11*	*7*	*14*
Special education (gifted, autistic, deaf, learning problems)	*7*	*7,5*	(2)
Administration and management	(5)	*6,5*	*10*
Research methodology	*9*	(1)	(0)
Schools and careers	*6*	(3)	(2)
Theory of education (including history of education and comparative education)	(2)	*7,5*	(0)
Evaluation	(0)	(1,5)	*10*
Culture/discourse	(3)	(1)	*8*

3. As far as I know the *South African Journal of Education* is the only subsidy-drawing journal in education in SA.

4. HSRC Newsletter *Education - Priorities for Research* Pretoria, Human Sciences Research Council, October 1988. This Research Newsletter consists of seven un-numbered pages of text in Afrikaans, and (translated) just over six pages of text in English. I quote freely from this document in what follows without providing page numbers.

7

Recent trends in the theory of culture

Robert Thornton[1]

Theory and methods

After the sciences of nature somehow wriggled out of the harness of a general moral and theological holism and became the "natural sciences," the social sciences have struggled to drag forward the baggage of a commitment to what is today called "grand theory."[2] As we approach the twenty-first century, at least some of this baggage has suddenly become heavy. It is sometimes difficult to see why we have dragged it so far. Looking at it closely, some say that the grand theory of much of contemporary social science is often little more than the accretions to or elaborations on the grand philosophies of Kant, Hegel, Marx, Nietzsche, and Durkheim, among others. We wonder: is there more to be added to the load? Can it be improved, repacked, relabelled, re-sold, revised? Should we cut and run?

Meanwhile, the natural sciences have long since divided their loads, improved the designs of their harness, denied their own origins in these same grand moral philosophies and, apparently, now go their separate ways. While maintaining a commitment to their baggage, the social sciences have always wondered why they can't wriggle out of the harness too. Perhaps there was a trick - a method if not a methodology - that would permit them to do what the natural sciences did.

Speculation has focussed on the nature and structure of the baggage, on the construction and design of the harness, and the problems of pulling this old cart into the future. But wait! Perhaps there is no baggage, no harness, no horse! At the current moment, it sometimes seems that we social scientists have suddenly noticed that we are out on the road, alone, but for our metaphors ...

Before we go too far with this, however, I want to say that I think there is a "road" - the imperative to understand who and what we are - and I think we are really real. This "reality" that we attribute to our understanding need not be

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objective truth, however, for it is after all we who decide whether and what is real. That judgment, moreover, is necessarily partly moral, partly political, partly intellectual (or cognitive) as well as experiential/experimental (or scientific). While such judgments are not simple, they do permit us to decide, from time to time, who and what we are as well as that we are.[3] The criteria we use to make these judgments are inexact, mutually contradictory and usually implicit or "tacit" - that is, they are unspoken. But it is precisely the tacit quality of these criteria that makes the distinction between Science and Social Science seem so urgent, yet be so fraught with enigma and paradox. It also permits a paradoxical strategy in which Science makes its rhetorical claim for a special kind of legitimacy by showing that it is concerned with the other-than-human, while Social Science seeks status by claiming to be like Science!

Balancing between the positions of contemporary philosophical realism (Outhwaite, 1987; Sayer, 1986) and pragmatism (Rochberg-Halton, 1986), I believe that there are real entities at the level and scale of society and culture, but join with a general skeptical feeling (which is not yet fully skeptical in the philosophical sense) that current concepts are somehow wrong.

My sense of disquiet thus springs from an uncertainty about finding an appropriate way to understand "us" from the point of view of "we". The post-colonial condition means, increasingly, that there are fewer and fewer people who can be labelled simply "Them" without fear that they will also clamber out of the undifferentiated sea of nature and demand a place under the pronoun "we". Some have attempted to capture this sense of disquiet which looking at ourselves look at ourselves occasions with the label "reflexivity", or "reflexive" anthropology.[4] This disquiet is not new[5], and it suggests another old metaphor: the mirror. This metaphor is already familiar to anthropologists who have for some time believed that anthropology holds the mirror for Man.[6] The "mirror" was the ethnography that anthropologists wrote in order to reflect the diversity of human behaviour and beliefs. It did work: Mankind has truly seen itself in this mirror and does not always like what it sees. Sometimes anthropology has been blamed for this reflection, and sometimes it is Mankind or "human nature." Reflexivity, by examining the relationship between the anthropologist and his subject, tilts the mirror or holds another one up so that the anthropologist now sees himself looking at "Them." "They", those "Others", of course, have always seen the Anthropologist looking, but now we see the Anthropologist looking at himself look at Them.[7] Is this just a bit foolish, or does this really get us some way along the road?If the current trend to reflexivity is just a mirror trick, it is still nothing more than a playful metaphor (see, for instance Watson, 1987; Chock, 1986). In terms of the partial allegory of the horse and harness with which we began, this kind of reflexivity consists in simply looking over our shoulders at the burdens of the past while regretting the harness that binds us to them. Although for many current writers, the idea of reflexivity is little more than this, the notion probably fails to conceptualize adequately the full implications of the idea of reflexivity in research. Several recent essays and books have attempted to explore these implications such as *Anthropology as cultural critique* (Marcus and Fischer, 1986), *Writing culture* (Clifford and Marcus, 1986), *The predicament of culture* (Clifford, 1987). They have succeeded in refocussing attention on the techniques and meaning of cultural relativism, the significance of power-relations in social research, the methods and means of writing and translation. They have also undermined many claims for "objective" or absolute truth. There is now an intense mutual fascination between literary theorists and social scientists as they struggle to understand the nature of the relationship between "truth" and "fiction."[8]

While many of the old metaphors are being abandoned, new conceptions which build on ideas of time, process, power, knowledge, representation, and writing have taken their place. All of these new conceptions have in common the insistence that the observer is also part of the social situation he observes, a renewed concern with context and the details of how things actually get done rather than with the larger conceptual wholes that we have so far been calling "society" and "culture" (Ortner, 1984, Bourdieu, 1977). What actually happens "out there" in the field is now called "practice", and the idea of society, or culture is more and more being thought of as the sometimes unintended consequence of these practices.

The differences have been summed up under the labels "Modern" and "post-modern." (Lyotard, 1988 [1979]; Tyler, 1986, Jameson, 1984; Baumann, 1988). Indeed, the most significant recent trend in cultural anthropology is the formulation of a diffuse but powerful programme for a "Post-modern Anthropology." While neither "Modern" nor "Post-modern" approaches can be precisely defined, they partly define each other by their opposition (Rabinow, 1985; Eagleton, 1985; Kolakowski, 1986).

"Modern" theories of society and culture are understood today to be those which represented society and culture in more-or-less pictorial ways: their object was imagined as a "whole" in which the parts "functioned", "articulated", were "structured", or just existed, more or less timelessly but nevertheless within history.[9] In modernism, history is simply assumed, that is, it is a more-or-less un-theorised part of the heritage of the nineteenth century's concern with evolution and progress (Giddens, 1986: 228-244). History has been schematised and "modernised", however, to the extent that concepts like "tradition" or "the past" no longer have theoretical force (Shils, 1982), but simply exist as part of the scenery going by the windows of the Now as it chugs relentlessly into the future. Modern theory is schematic and categorical, presented "from the outside" as if the observer is not part of the object of his observation; as if s/he never observed it at all but simply pointed to it through the transparent medium of the text. The aim of the "Modern" study of culture was to achieve an empirical "theory" that generalized about laws of culture in a way similar to the theories of the natural sciences. Its principle metaphors for dealing with social life were organism and language (M. Jackson, 1987; Thornton, 1988b). Both organisms and languages were understood as functioning wholes within history but largely immune to historical influence,[10] and as such, provided modern analogies for working out the implications of what continued to be essentially nineteenth century questions about progress, social class and other "differences", and about the integration or disintegration of social wholes.

By contrast, the Post-modern understanding of the study of culture notes that the "holism" of previous views of culture and society was literally illusory: it depended on a set of visual metaphors and particular rhetorical techniques that convinced readers of its veracity, but which could never be empirically demonstrated (Clifford and Marcus, 1986: 11; Thornton, 1988a; Tyler, 1984; Giddens, 1986: 236). Furthermore, the post-modern recognition that the study of culture is always inevitably conducted from within culture, prohibits the fiction of exteriority - the idea that culture can be understood and represented from the outside - and thus makes the study of culture unique among the methods employed in the search for knowledge. In particular, this new view precludes theory, since theory is necessarily an exterior knowledge, while it permits only methods, procedures which allow us to grasp better the ideas and resources which constitute culture but which can not provide the exterior and coherent picture that theory provides.

Post-modern emphasis on methods does not entirely replace Modern theory, although it does make Modern theorists very nervous indeed (for example, Yengoyan, 1986; Shankman, 1987). It contributes a set of new concepts, new questions, and a self-awareness of the context and techniques of writing as a situated method for representation and interpretation.[11] New emphasis is placed on the diversity, internal contradictory-ness, and uncertainty of cultural knowledge, while re-focussing on the central place of time, process, social power, representation, and reproduction in the effort to understand the nature of human culture. The principle metaphor for society in the post-modern way of thinking is the text, conceived as a self-referential system of signs and "differences" (Clifford, 1988: 38-39; Jackson, 1987: 17; Tyler 1985). Nevertheless, within postmodernism, culture itself is a significant problem. Clifford speaks again and again of "the predicament" of culture (Clifford, 1988), and with considerable precision in pinpointing the spirit of the times remarks that "culture is a deeply compromised idea I cannot yet do without."

Whole cultures, part-cultures and culture

In *South African Keywords*, I present the view of "culture as a resource", and reject the idea that cultures can be treated as fully bounded wholes (Thornton, 1988).

In that discussion, I argued that culture (rather than Cultures) is a set of resources and practices which involve those resources. This formulation moves away from the "deeply compromised" notions of culture understood as the single evolving product of human progress (after Hegel, Marx, Herbert Spencer, Matthew Arnold and James Frazer) or the property of bounded social units that "had" culture more or less as they could be said to possess a language.[12]

The idea of discourse, or better, discourses, is an example of a concept that improves our ability to understand "us" from the point of view of "we". Indeed, discourses, considered as resources of culture have some of the same kinds of continuities and "reality" that we have previously linked with Cultures (that is, the idea of bounded, coherent and named entities). The discourses which are now being "discovered" by historians, anthropologists and psychologists rely, of course, on other kinds of meanings, and achieve their coherence from the context in which they are practiced. The notion of discourse privileges the speaking subject, and the immediate situation of speech, namely the pragmatic context in which the subject acts, thinks and plans. Unlike the structuralist who derives the meaning of the situation from the comparison between the context and the subject-less global structure of language, those who rely on the idea of discourse seek the significance of events in the coherence of the moment, the pragmatic "logic" that links multiple voices, plans and outcomes in the patterns of "everyday life". In order to understand the levels of grammar, syntax, and semantics on which such cultural meanings rest, however, we must rely on the formal methods, already well established in the human sciences, for analysing discourse into its elements: symbols, icons, indexes, symbols/signs, turn-taking, pragmatics, rules of use, and so on. These methods emphasize the detail of context while they fail to provide an exterior knowledge or theory-like picture. Modernists believe this is a fault; post-modernists see it as a virtue. While they have much in common as techniques with earlier formal techniques of analysis, language in context - what Saussure (1966 [1915]) called *parole,* or language as it is spoken and used in daily life - is no longer seen to be the imperfect instance of globally transcendent "perfect" languages (Saussure's *langue*) nor is it seen as fragments of coherent meaning systems that "exist" in the group mind, *conscience collectif*, or Cultures. Rather, what is observed is taken to be all that is the case (to paraphrase Wittgenstein), with its lies, contradictions, flawed logic and foolishness integral to the human reality.

This method and approach to understanding culture provides us with the tools for understanding our own practices and understandings. Issues that can be approached from the viewpoint of such a theory of culture include (not surprisingly) current political debate (The "own affairs" notion in the current absurd constitutional "dispensation" in South Africa, for example; see Boonzaier and Sharp, 1989), gender and sexism, race and racism, popular culture (What do Brenda Fasi and Madonna have in common? What are their differences? And their audiences?), and history.

I advocate, then, a particular view of culture: Culture is a set of resources which consists of objects, sounds and languages (one or many), traditions, customs, habits, beliefs and so on, but also includes secrecy, violence, deceit, revolution and innovation - all essentially and constantly self-undermining and mutually-contradictory. Somehow, in trying to understand "the ways of being human," we have managed to forget that the ways of being inhuman are also part of culture. But most importantly, the idea of culture is itself part of culture - indeed having an idea of culture, that is, possessing the notion that culture is what makes us human, is perhaps the *sine qua non* of having culture at all. It is this reflexivity of culture - the knowledge that the idea of culture is part of culture - that is the most important thing that current thought has begun to teach us (Thornton, 1988b).

According to this view, even the idea that people live in unique and isolated Cultures (The English Culture, the Zulu Culture, or even the Western Culture or African Culture) is also a part of the category that we call "culture". We can see that this is true primarily because we can trace the history of these ideas and see that these ideas emerge at a particular time for particular reasons. Furthermore, not everyone accepts these ideas as true, nor is it possible to fit all individuals into one or other category of this type.

There are other significant implications of this insight. Culture can not now be seen as "all of a piece" or even very systematic since different people or groups of different people know different things, have different degrees of access to the resources of culture, and use those resources differently. The idea of different "discourses" is useful in talking about these differences.

A good example is the usually stereotyped but often eloquent and moving speech acts which we call "literature", "poetry", "oral literature", "oratory" or "ritual speech". It exists in all human communities and situations. Only some people, often very few of them, "do" it, "have" it or "say" it, while many more claim it as uniquely theirs, as their language, their national or tribal essence. Nevertheless, its most salient feature is that, while it is ascribed to everyone, moving most and mystifying some, it is always exclusive, jealously guarded, hedged by sanctions or personal claim backed up by threat of violence: in short, it is emphatically not everyone's, (often) not for women, not for children, or enemies, or competitors (Keesing, 1987b; Ginsberg, 1987; Marcus and Fischer, 1986). Culture is controlled, distributed, owned, restricted, given and taken.

Consider the "oral literature" in Sotho that has been ably described by David Coplan (Coplan, 1987). The *lifela* oral poetry in the Sotho language, is a discourse - that is, it is a set of resources with continuities with the past that provides the present with ideas and forms of expression. Nevertheless, not all Sotho speakers know or understand this particular use of language, and people who do not speak Sotho as their first language may know, or learn about them, either in translation (as we read about it here or in Coplan's work, for instance) or by hearing it in Sesotho (even though their "own" language may be Xhosa, English, Zulu or Shangaan). Even though we might speak of it as "Sotho poetry", we must recognise its specific focus on the historical experience of particular people who share some characteristics: male Sotho-speaking migrant mine-workers, and some Sotho-speaking women associated with the migrant workers. We do not then fall into the stereotype of supposing that all Sotho-speakers are poets or migrant mine-workers, but instead have a basis for understanding and describing the history of southern African culture as it grows and changes (Coplan, 1987; compare Sherzer, 1987: 295-297).

Thus, from the point of view of some contemporary anthropologists, it appears that even if all people of any social group do in fact (that is objectively) share the same experiences and environment, no two individuals have precisely the same understanding or memories of it. Moreover, if they do (somehow) have the same objective "culture" they certainly are not able to report it with any degree of accuracy.

According to this view, which is supported by many fieldwork studies, statistical studies, and controlled psychological studies, culture is fundamentally diverse, and consists not of a single image or view but rather of a multiplicity of images and views. Studies by Keesing, Comaroff, Coplan, and Ginsberg (whom I shall discuss below) illustrate this in a number of ways, drawing on North America and South America, Solomon Islands, and South Africa. These studies illustrate the point by showing how cultural knowledge is presented in narratives and stories that never present a whole "objective" truth, but only partial versions. Differences of age, gender, life experience, perspective and other contingent facts structure reality much more forcefully than do reified theoretical entities like "society" or social structure.

The diversity of culture and the problem of studying it

Keesing (1987a) notes that the study of culture has been taken to be primarily an "interpretive quest". In other words, anthropologists and others who take the study of culture to be the core of their academic practice have generally attempted to interpret, explain, illustrate and clarify ideas and concepts from many sources. In *Anthropology as interpretive quest,* (Keesing, 1987: 161-168), Roger Keesing summarises a number of contemporary complaints and problems with this view. For Keesing, the chief problem arises from the view that cultures are shared systems of meaning or collective phenomena. This is a problem, he argues, because cultures as collective phenomena, as symbols and meanings, as public and shared, needs to be qualified by a view of knowledge as distributed and controlled. Even in classless societies, who knows what becomes a serious issue.

An anthropology that takes cultures to be collective creations, that reifies them into texts and objectifies their meanings, disguises and even mystifies the dynamics of knowledge and its uses (1987: 161). Keesing is especially concerned about the idea of cultures as "wholes". While a great deal of knowledge may indeed be public, collective and shared, there is also a great deal that is not. Both shared and deliberately not-shared knowledge is cultural. We need only think of racially or ethnically divided societies in which access to jobs or specific skills is restricted to members of social categories or groups. Even in the class-less society of the Kwaio in the Solomon Islands in which Keesing worked, knowledge is restricted according to age and gender.

There is a problem with Keesing's presentation, however, since he seems to assume that there are, after all, such things as "cultural wholes" and that the problem is whether or not they are shared and collective. The possibility exists, for him, that there may be multiple cultural wholes - "women's cultures", "men's cultures", "children's cultures" - within a single society or group. He does not directly confront the deeper issue of whether there are such things as "cultural wholes" at all. The argument put forward in Thornton (1988b), however, treats culture as a set of resources which share some cultural characteristics (that is, we recognise them as part of culture, not biology or physical environment) and which are employed differently by different individuals in different contexts.

But Keesing also takes issue with idea that cultural knowledge is like a text. He argues that while a text (a novel, a poem, a political speech) can be translated and interpreted, and thus made clearer and understandable, this is not sufficient. If cultures can be seen as texts, they are "deeply ambiguous, allowing alternate interpretations" (Keesing, 1987: 162). Because of this, the analogy of text is not adequate.

Keesing recommends a study of culture as knowledge, but knowledge which is controlled by different people and by the power they hold over each other and by the economic resources they command. Keesing's criticism is consistent with the "post-modern" critique of the modernist ideas of culture that emphasized "shared knowledge", "norms" and "public behaviour." Although Keesing highlights a number of critical and important problems in contemporary cultural studies, there is still the tantalising problem of how we (as observers, anthropologists, or even as garden-variety "members of the society") can have access to cultural knowledge.

A number of recent studies have shown convincingly that individuals can not be relied upon to tell us very much about what they actually believe, see, experience, or hold to be true. This calls into question the apparently obvious idea that cultures are shared, public and collective. If this were true of cultures, then, it would be expected that most individuals should "know" in some form or other, what their customs, beliefs and experiences are. Interestingly, it turns out that they don't. This can be explained in two ways. First of all, as Keesing notes, knowledge is controlled, subject to social power and differentially distributed among classes, genders, families and individuals. Secondly, people do not in fact accurately remember or report what they have experienced. On average, they are wrong about half the time (Bernard, Killworth, Kronenfeld and Sailer, 1985).

Thus even if culture was in fact (i.e. objectively) shared, we as observers (or even as participants) could not achieve accurate access to very much of it, nor have any confidence that what we do know is "correct". Instead, it turns out, people operate in terms of patterns and templates that structure the knowledge they do have, and control its use. Thus, knowledge - and I believe we can extend this to other aspects of culture - is doubly restricted and controlled: by the social structures in which we live, and the cognitive structures that organise the mind (Freeman, Romney and Freeman, 1987).

Bernard *et al.* (1985) reports on diverse studies that measured what people said about what they did compared with what they actually did (or compared with what others observed them doing). These studies ranged over areas of health care (their own and others), child care, diets, communications, and other areas. On the average, over most studies, informant accuracy (or inaccuracy) was measured at about 50 percent. In other words, informants were wrong about their own behaviour, beliefs or experiences about half the time.

Subsequent studies have refined this initial finding, however, and show that there are other factors that make the 50% inaccuracy figure too simple. The Freeman et. al. (1987) study shows that if informants are divided into two groups of "worst informants" (i.e. those who provide the least information) and "best informants" (those who provide the most information), two different patterns emerge. The "worst informants" provide accurate observations which are incomplete. The best informants provide inaccurate observations, but their knowledge is based on cultural patterns of long-term behaviour or experience. The inaccuracies of the least-informative informants are due to what they have forgotten, what they leave out, and what they have failed to observe. The inaccuracies of the most informative informants on the other hand, are due to what they have "invented" in order to make their reports consistent with what they have experienced as general patterns. When the two kinds of errors are summed and averaged, the gross error rate is about 50%. Practically, speaking, however, in the field where we can rarely know the precise truth except through what people say, we do not in fact know what kind of informant we have, so the average error of 50% appears to be an accurate index of the reliability of what people tell us.

The most significant implication of this is that even if "culture" is completely and wholly shared by all members of a "society", in practical terms we can not know this! The conclusion we must draw from this then is that the idea of "culture as shared" has practical difficulties as well as conceptual difficulties.

The significance of the idea of "discourses"

Having shifted our attention, then, away from the idea of studying "whole cultures", anthropologists are today concentrating on what are often called "discourses" and "practices". Sherri Ortner, in an influential article published in 1984 attempts to summarise the direction and changing concerns of Anthropology "since the Sixties." In her view, the focus is currently on the idea of "practice" - that is, consideration of what individuals take to be the point of what they are doing, how they do it, and how they understand it. Many articles currently appearing in the anthropological journals reflect this approach.

Coplan (1987) for instance examines the "discourse" of Sotho poetry. In his study, he is not simply concerned with aesthetic criticism, interpretation, translation, or history of Sesotho poetry, but rather with the practice of poetry in its historical and political context. In other words, he shows us who the poets are, what their experience of migrant labour and rural life have been, how they are related to others who compose and recite poetry, and to their audiences. Here, poetry is not treated as texts to be read, but as a discourse, that is a particular form of speech with a particular pattern, style and content. Importantly, it also has a history and significant relationship with aspects of social structure such as gender (men and women), age (young migrants vs. "the old folks at home") and class (labourers returning from the mines, shebeen owners, cattle owners, etc.). This kind of poetry then is not "just language" but a particular form of social practice. Significantly, Coplan uses a combination of text, videos and sound-recordings to present his findings. Coplan shows that it matters who its creators and its audience are, that there is an historical and social pattern, and that it transcends what we might previously have called "Sotho Culture".

Ginsberg (1987) is another example that treats a different kind of practice in a different way from the standard sociological approaches that we might expect. She studied a situation (the opening of an abortion clinic), and the development of political action groups of women that organised around the issue of the clinic to press for different political outcomes (the continuation v.s. the closure of the clinic). Ginsberg's approach is significant here. Instead of focussing on the social structure of the opposing groups ("Pro-life" as opposed to "Pro-choice" activists), or on the classification of their memberships according to the standard sociological categories of economic class, religion, level of education, ethnicity, and so on, she chose rather to look at the "life stories" of the women who were involved. This approach revealed that the membership of the two groups could not be easily explained in terms of the class, ethnicity, religion or other "standard" sociological category of the individuals. In other words, although we might expect that all middle-class Italian Catholics would support "Pro-life" (anti-abortion), and all upper-class Protestants would be "Pro-choice", this in fact was not true. Instead, she found that membership depended on the historical and personal experience of individual women. This finding argues strongly in favour of a practice oriented approach to social research since the significant sociological factor was the experience and self-concept of individuals rather than the pattern of standard social categories. The practice that Ginsberg is especially concerned with is the practice of being a woman in a contemporary American city, and the struggle that these women feel between being nurturant reproducers of other human beings and being turned into "structural males" in the work-place.

Comaroff and Comaroff (1987) take a similar approach to the "discourses" of a Tswana "madman" and other migrant labourers in order to show how their historical experience is not expressed as a "class" or even as a group, but in the individual expressions of resistance and identity in often idiosyncratic ways. Thus, the madman's costume is understood not as a psychological aberration, but rather as an index of the historical experience of an individual. The Comaroffs concentrate on a contrast that they see as especially important to the Barolong Tswana people whom they worked with. This contrast is that between "work" and "labour" which they chose to see as different practices that are represented in different ways. "Labour" is the practice of migrant labour to the mines on the Reef, while "work" is the practice of reproducing and producing their own livelihoods in their own homes. The article shows that this contrast is expressed in many ways in the regional culture. It is one of the"resources" (here, a set of ideas) that are used to understand their own life experiences.

Unfortunately, in my view, the Comaroffs maintain very strongly the view of Cultures as unique and separate. They speak of a "Tsidi consciousness" which they seem to present as wholly different from some other kind of consciousness. It appears that the Comaroffs contrast the "Tsidi consciousness" with what they call the "Western" consciousness. They seem to forget here that in terms of scale, history, and specificity it is virtually impossible to compare a "Western consciousness" (Whose? The chairman of IBM or a Spanish grape-picker?) with the "Tsidi consciousness". The article illustrates both a sensitivity to the experience and "discourse" of Tswana migrant labourers while at the same time showing how difficult it is to break out of the old "Different Cultures" view. Current social theorists, especially feminist and Marxist theorists, have pointed out that the understanding of the "culture" often reflected only the view of the dominant, usually male, members of the group. Women often had little access to this knowledge. Indeed, the recognition that cultural knowledge of certain kinds was often specifically and deliberately denied to women, children, some junior or not-elite males, and so on, raised a big question mark over the idea of "culture as shared" in the first place. Keesing (1987) and Pratt (1986) give examples of responses in contemporary anthropological practice to this realisation, and illustrate some of the results that have been achieved once this realization was taken seriously.

Ideology

Contemporary methods of cultural analysis seek to organise and interpret diverse perspectives in order to give an account of what it is to be human. Inevitably, the only unity we find is the unity of context, experience, history, time (or events) and space (or place); in other words, we find a contingent unity, which we must describe contingently.

Ideology, by contrast, constructs a unity for its subject matter. As such it may provide the outlines of political action and intellectual comprehension, although anthropologists would assert (and, increasingly, others in disciplines such as literary theory/criticism) that it can never be the only such outline. The unities and systems that anthropologists discover in the lives they study, then, are crucially dependent on constructs such as ideology among others.

The word itself, ide/ology, suggests a simple analogy with the word anthropology. The comparison suggests that the former would mean "the study of knowledge of ideas", as Anthropology is the "study or knowledge of humanity". But, revealing nothing in itself, this analogy draws our attention to what anthropology has taken to be central in ideology. Let us take more seriously, for the moment, the original meaning of the Greek word logos, from which -ology is derived. For logos meant not simply "word", but, more than this, its meanings extended to "the power of the word", or the "universe of speech" - in short, discourse and rhetoric whether from god(s) or from men. And it is this which anthropology has, in large measure, taken as central to the analysis of ideology. If ideology is anything, it is language, discourse, sometimes a kind of myth or rhetoric. With Wittgenstein (and not from Wittgenstein, since anthropologists were doing it already), anthropologists since Frazer, Boas, and Malinowski have been concerned with the function of ideology as a contingent discourse about the contexts in which people find themselves. As such, they have striven to understand it comparatively, and to explain how it shapes and is shaped by the cultural universe and psychological orientations of a diverse humanity.

Contemporary discussions of ideology have all focussed on the fact that ideology is embedded in language, that it can have no other sense than as an aspect of language, and must be understood as part of the general study of linguistic formations. Ideologies are spoken and written, they are learned as part of the process of learning language itself, or at least as part of learning certain realms of restricted discourse within the vast domain of language.

Unlike other social scientists, anthropologists have understood ideology to be an aspect of language with political functions rather than as an aspect of politics which is expressed in and through language. Of those who have informed the anthropological study of ideology, probably the most fruitful ideas have come from those scholars, whether within anthropology or not, who have taken the study of language itself as the high-road to human understanding. The central place occupied by language in Anthropology can be traced back to the nineteenth-century concern with the authoritative text of the Bible in classical philology, especially as this concern was transformed by Max Muller and James Frazer into a concern with myth as text, and then, following Malinowski, into a concern with text-as-charter for social order as well as for social chaos and dysfunction. As the intellectual descendants of Bronislaw Malinowski, Franz Boas, Benjamin Lee Whorf, Ruth Benedict, and Claude Levi-Strauss, contemporary anthropologists have pushed the understanding of language in ideology even further. Very strong influence from outside the discipline has come from Max Weber (in the English-speaking world, especially through Talcott Parsons and Edward Shils), and more recently from Lukacs, Mikael Bakhtin, and Michel Foucault. All of these scholars have been committed in their own ways to understanding the role of ideology in their own societies. The dialogue which has taken place between this interest in ideology in European and American societies, and the concern with the ideology of other, often very different and usually non-Western societies, continually informs our views of ideology in general. This dialogue, too, contributes a particularly anthropological insight to this study.

A distinctive anthropological view of ideology can be considered under several rubrics: ideologies as quasi-aesthetic wholes, as "cultural systems" of "local knowledge", as types of social romance, a special kind of complex trope, or as text. The view that runs through them all is that ideologies are just a special kind of discourse - albeit privileged and sanctioned - among many other kinds of discourse.

Some cultural anthropologists, especially American ones, have written about ideologies as aesthetic wholes. Ruth Benedict, one of Margaret Mead's most pensive and literary students, seems to have understood ideologies in this way. Her study of the Japanese ideology, motivated by the events of World War II emphasised the aesthetic commitment to symbols, in this case the chrysanthemum, an ancient symbol of Japanese national identity and Phoenix-like transcendence, and the sword, the symbol of manly honour and the warrior's ascetic ethic. This approach emphasized the sense of wholeness and purpose that a national ideology contributed to the action and commitment of individuals, and like other sorts of holism in the social sciences, closely identified the macrocosmic cultural system with the microcosmic integration of each individual's psyche or personality. In this way personality, the integrated psyche, became the testing ground for hypotheses about the ideological systems which it was believed to reflect or instantiate. Benedict's view was related to the pragmatic psychology of William James, and to the dramatic psychology of Friedrich Nietzsche, and marks a mid-point in the development of theories about the relationship of large scale ideological systems and their realization in individual behaviour and worldviews.

The view which sees ideology as an aesthetic whole has conceived of it as essentially conceptual and image-like, rather than practical or technical. Such approaches are firmly modernist in character. Recent studies of ideology in this mould include Donald Levine's study of Ethiopian royalist ideologies in the feudal social system of Ethiopia in the 1960's before the death of Emperor Haile Selassie. This study, entitled *Wax and gold* (1972), relies far more on Max Weber's concepts of tradition and authority, but develops these concepts in terms of the royal images of bees-wax, gold, the Queen of Sheba, and the icons and symbols of Coptic Christianity. The aim is to understand the legitimation of a twentieth century feudal system from the Ethiopian point of view, that is in terms of their apparently high aesthetic valuation of a holistically conceived "Ethiopian-ness" expressed in artifacts as well as discourse. Ideology, Levine argues, can scarcely be separated from the entire integration of religious practice, ethnic identity, and the authentic Ethiopian personality.

Studies of ideology such as those of Benedict and Levine, exploit in their writing the aesthetic appeal of objects and language which appear to account through their own inherent qualities for the appeal and power of ideology itself. Rhetorically, their works are characterised by clearly defined texts, closed and finite. Their holism is not in itself directly available to experience, but is presented as a "national personality type" in Benedict, or as a Weberian "ideal type" in works such as Levine's. The fundamental place of language as behaviour or as text is missing, as is a concern with the role of power and coercion.

Partly developing out of the cultural holisms that dominated anthropology up to the sixties, but also drawing heavily on hermeneutics is the treatment of ideologies as cultural systems. The phrase itself is from Clifford Geertz's work. In a series of related articles entitled, for example, "Religion as a cultural system" (1976), "Ideology as a cultural system" (1976), or "Common sense as a cultural system" (1983: 71-93), Geertz develops a method of interpretation of ideologies, religion, common sense, and other things such as law or art, in which the aesthetic sense of wholeness persists, but to which is added a theory of how these conceptual systems actually can be said to "work". Like religion, art or "common sense", ideology is a system of public meanings, formulated in the daily business of going about one's business. These meanings, at least in societies which have not yet been suffused with the homogeneous homilies of the mass media, constitute "local knowledge":

> local not just as to place, time, class, and variety of issue, but as to accent - vernacular characterizations of what happens to be connected to vernacular imaginings of what can. It is this complex of characterization and imaginings, stories about events cast in imagery about principles, ... This is doubtless more than a little vague, but as Wittgenstein, the patron saint of what is going on here, remarked, a veridical picture of an indistinct object is not after all a clear one but an indistinct one. Better to paint the sea like Turner than attempt to make of it a Constable cow (Geertz ,1983: 15).

It is local knowledge, publicly, and symbolically constituted, that defines the world, and ensures the myriad misunderstandings and contradictions which that archetype of ethnography, the Bible, accounted for with the myth of the Tower of Babel, and which are still responsible for the fact that ideology, religion, language and other such meaningful constructs are seen as serious problems. What ideology does (like religion), is to provide a set of explanations - cathartic, moral, solidary, or advocatory - and to imbue these with what he has called "an aura of factuality", that is, to present one version of local knowledge as somehow uniquely real. For Geertz it is the system of symbols in language, dance, art, oratory, daily conversation, play, work, etc., which act on the psyche to motivate action and to ensure that action is conducted within the local frameworks of appropriateness. Ideology, for Geertz, is a performance. Others have understood ideologies as a type of social romance, or complex trope through which reality is comprehended and represented. By "romance", we mean here a narrative with the distinctive hallmarks of textuality - the concrete material existence of texts - on which all other subsequent narratives depend. By representation, we mean an image of reality which is not the same as that reality, but which nevertheless is itself part of reality. The idea of representation, then, involves a concept of an idea which is an aspect or part of itself, and so operates recursively on itself. The unities conceived of are always contingent on the concrete instance of texts which embody them - but only for the moment since they are part of historical processes that transform themselves.

Michel Foucault (1972: 4-8) distinguishes between an historical sensibility which takes as its subject matter vast "periods", and "centuries", and one which focuses on the discontinuities, the "strata" and "silences" of the historical record in order to achieve a more limited understanding of individual works, texts, oeuvres, institutions, or social formations albeit within the historical process.

Louis Althusser, for example, is concerned, at the most general level, to understand the discontinuities of the western ideology which "establishes a science by detaching it from the ideology of its past and by revealing this past as ideological" (Althusser, 1969: 168). Marx's own statement that "the history of all hitherto existing society is the history of class struggle" is just such an enunciation of discontinuity between his text and the "text" of "all hitherto existing society". Althusser's account of ideology, and of ideological formations is similarly concerned with detachment, formation and objectification rather than with continuity, causation, and subjectivity. It is a major influence in those parts of contemporary anthropology that concentrates on the discontinuities of class "formations" in societies which have been treated, up to now, as radically continuous and solidary. But Althusser's own method, however, focuses on Marx's text itself, and emphasises the discontinuity between this text and all other texts which existed beside it and, in effect, within it. This approach generates a fundamentalism which, in its insistence on one text as ultimate source of knowledge, is not appreciably different from religious fundamentalisms.

Before his own madness and silence, Althusser believed that his own literary re-structuring of Marx's text constituted a radical discontinuity from other similar literary analyses, re-constructions, or de-constructions. That is, he believed (as Marx himself seemed never to believe) that he had created a science. But, as Foucault points out what distinguishes science from ideology is simply different "discursive practices" that constitute, formalise and enunciate the world in different ways (see also Lyotard, 1979).

Finally, and overlapping with all of the other views of ideology within anthropology, we have the view of ideology as authoritative discourse. Here language is taken not only as the privileged mode of representation but of coercion as well. This view takes seriously the notion of discourses as essentially and always contradictory and self-undermining, and is therefore the most "post-modern" in character.

Talal Asad and James Clifford, for instance, have argued that in general anthropology has appropriated the "authoritative discourses" of other societies into its own authentic text. In so doing, the conflict, contradiction and incommensurability of discourse within the contexts where anthropologists have worked (we may not say "societies", here, for this introduces a spurious unity) have been homogenized in order to present them as "myth" which maintains or justifies the social structure. The essential dialogic and dialectic nature of ideology in practice is lost, then, as it is subsumed in a general theory of knowledge as function. In Anthropology, according to Asad, "the possibility of such a reduction is located in the absence ... of an adequate understanding of authoritative discourse - i.e. of the materially founded discourse which seeks continually to pre-preempt the space of radically opposed utterances and so to prevent them from being uttered. For authoritative discourse we should be careful to note, authorises neither "Reality" nor "Experience" but other discourse - texts, speech, visual images, etc. which are being structured in terms of given (imposed) concepts, and reproduced in terms of essential meanings" (Asad, 1979: 621).

These approaches to ideology differ in the degree to which they accept the possibility of an exterior, non-situational or non-relativist theory is possible. Some, like Althusser (and Lukacs and Gramsci on whom he relies), have equated ideology with culture, while others treat it as merely a resource among others. The second view is most consistent with the understanding of culture that I present here.

Conclusion

Theory in anthropology today has been celebrated, worried over, and dressed up. Now we wonder: Where will it go? It has isolated itself by purporting to be an abstract and exterior perspective that does not permit a flexible and pragmatic reaction to the complex realities of everyday life. This has been common cause for some time. In my little story at the beginning of this essay, I indicated that the way out of this impasse has been sought, unsuccessfully, by attempting to follow the physical and natural sciences out of the harness of totalising and holistic moral philosophies or theologies. This has worked for them because their subject matter is not, after all, morally constituted but exists independently of human consciousness of it.[13] This move cannot work for the social sciences because their object is morally constituted. The methods - or tricks - of the physical sciences have almost nothing to contribute to the social sciences.

Paradoxically, however, the resort to theory may still be a useful move. If it allow us to consider aspects of human behaviour and thought temporarily removed from the context of moral evaluation it may permit insights which we would not otherwise obtain. Although temporary, this is a valuable method in the search - or creation - of knowledge.

But a temporary escape from the context of moral and political evaluation - just long enough to think clearly - is not equivalent to the natural scientist's resort to the laboratory, however much the methodological move may resemble it. It is nothing more than a very convincing metaphor. It is a useful metaphor, not because it functions like a good metaphor - that is, as a model which allows insight by making heuristic simplifications in the real object which it models - but because it is good rhetoric. In other words, we are right to call social science "science" not because it is a science like the physical and natural sciences, but because it is an autonomous form of knowledge, and because our association with the term "science" permits just that amount of institutionalized moral leeway that will allow us to think clearly about social, moral and cognitive issues. The appeal to science simply legitimates the methodological resort to relativistic consideration of aspects of thought and behaviour. Beyond that narrowly legitimated context of scientific inquiry - in life as it is lived, good and evil - we must return to the necessity of moral evaluation which motivated the inquiry in the first place.

Notes

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2. But not everyone takes theory so seriously. Note, for example, Gellner's remark that while Anthropologists insist that "Anthropology" differs from "ethnography" as theory differs from description, or as explanation differs from data, the "theory" of Anthropology (especially Functionalist Anthropology) "turns out to be in large part the doctrine that anthropology should be nothing but ethnography"! (Gellner 1973: 98) This remark can not, however, be applied to Structuralist, Marxist, or other more theory-laden versions of Anthropology.

3. This opposition of "who we are" to "that we are" corresponds to an evaluative judgment as opposed to an ontological one, but while the ontological judgment is absolute and strictly philosophical, the evaluative one is always contextual and pragmatic even though it resembles the ontological one. Until Nietzsche introduced the principle of power and perspective, the question of "who we are" was usually assimilated to the "that we are." (Strong, 1985).

4. This kind of reflexivity is characterised as a search for redemption by M. Jackson (1987: 19). He quotes Levi-Strauss, in *Triste tropiques* in support of his claim, thus: "The anthropologist is less able to ignore his own civilization and to dissociate himself from its faults in that his very existence is incomprehensible except as an attempt at redemption; he is the symbol of atonement" (Levi-Strauss 1973: 389; quoted in Jackson 1987: 19).

5. Marcus and Fischer point that there has been "a persistent oscillation between more realist modes of description and irony" in the twentieth century, with apogees of irony located in the 1920's and the 1980's (Marcus and Fischer 1986: 14).

6. The phrase, Mirror for Man, or some derivative of it, has been used by numerous anthropologists to describe the nature of their discipline, e.g. Milton

Singer, *Man's glassy essence* (University of Chicago, 1986); Clyde Kluckhohn, *Mirror for man* (New York, McGraw-Hill, 1949).

7. This is illustrated by several recent book covers. The cover of *Writing culture* (Clifford and Marcus, 1986) shows the young ethnographer (Stephen Tyler) in the field in southern India being observed writing field notes by a Indian informant who watches over his shoulder. Similarly, the cover of *Observers observed: Essays on ethnographic fieldwork*, (Stocking, 1983) shows Malinowski being watched intently by a dozen Trobriand natives as he sits at his typewriter in his tent.

8. This does not imply, of course, that "truth" is the only possible opponent to "fiction", that they are necessarily opposed, or that either "truth" or "fiction" are clearly defined unitary concepts.

9. The relationship between history and functionalism and structuralism, in particular, is a much debated question, not least by the chief architects of these twentieth-century theories. It is beyond the scope of this paper to enter into this question.

10. This overstates the point somewhat: eveyone acknowledges that languages and organisms do change, but on a much longer evolutionary time scale; thus, they are "immune to historical influence" by virtue of human (cultural) history's incommensurably shorter time-scale. See Thornton, 1989.

11. The term "theory" derives from the Greek theorein, "to look at" from *thea*, "the act of seeing" (Gk), and is etymologically related both to "theatre", the presentation of a spectacle or "imaginary" events and scenarios and to "theorem", the representation of abstract principles or propositions about mathematical truths. I use the term here in its widely accepted "scientific" sense in which theory is held to present economical explanations with predictive potency which are in principle refutable on empirical grounds. Methods, by contrast, provide explanatory procedures without predictive potential, and are assessable only on pragmatic, not empirical, grounds.

12. The term "method" derives from the Greek meta, "after, along, with" and hodos, "the way, a road." While recalling the horse-and-cart metaphor with which we began the discussion, this derivation adds weight to the pragmatic situated-ness of methods as opposed to theories.

13. The claim that "a culture" has "a language" is apparently persuasive, but only if history and social scale are ignored. For the European Romantic Nationalists (Herder, Goethe, Napolean, Garibaldi ...) to possess a common language was to possess "a culture", a common *Geist*, a potential nation. But common languages are imposed in the historical processes of power, politics and trade, and thus create common culture to some degree; they are not "natural". Regions (such as the island of New Guinea) possess common cultures without common languages (there are some 300 + mutually unintelligible languages spoken on New Guinea).

14. The theoretical constructs which explain it to us allow us to manipulate it in order to achieve our goals. Although the goals we set for technology to achieve are clearly part of culture (Sahlins, 1976), and the mode of legitimizing and justifying our choice of those means is necessarily part of language as discursive process (Lyotard, 1988), the mere physical means for achieving these goals are not.

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143

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Interpretation and rationality

8

Linguistic models and hermeneutic methods: Rethinking Dilthey through Gadamer and Ricoeur

Tobie Louw

The aim of this paper is to show the relevance of philosophical hermeneutics for reflections on the status of the human sciences. I shall, however, address myself less to the current debate on the "methodology" of the human sciences, and more to the hermeneutical dialogue sometimes taken for granted by the individual sciences.

With reference to the question of the relevance of hermeneutics for the human sciences, the following statements can be made. Not only are all questions concerning the methods of science basically philosophical questions, but they are at the same time hermeneutical questions. At the core of the philosophical contribution to the logical self-reflection of the sciences is the hermeneutical notion of *text-interpretation*. Far from being the brain-child of idle intellectuals, the "model of the text" represents an innovative philosophical reorientation of the scientific enterprise as a whole. In this context I would like to present Paul Ricoeur's argument (1982: 197) that the human sciences may be said to be hermeneutical, firstly in so far as their *object* displays features common to the constitution of a text, and secondly in so far as their *methodology* develops procedures akin to the interpretation of texts.

But more than that, the notion of the "model of the text" presupposes, and has its life in a philosophy of *language*, or more properly: of *discourse*. The aim of the latter is to account for both the recent developments in the linguistic sciences, and the developments in the debate on methodology. Inasmuch as science in general always stands under definite conditions of methodological abstraction, and inasmuch as possibilities for the acquisition of knowledge rests on the disclosure of new possibilities of questioning, every science is hermeneutical.

The primary sense in which the word "hermeneutics" is used, concerns the rules for the *interpretation* of the *written* documents of our culture. On one

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level, namely that of the theory of knowledge, interpretation only covers a limited category of signs, i.e. those that are fixed by writing. On another level, that of ontology, the procedure of interpretation assumes a concept of *understanding* that relies on the recognition of the meaningful forms that are handed down in a tradition. The latter includes the limited situation of *spoken* discourse through which persons express themselves in an unlimited variety of ways.

The ultimate concern of this paper is to reveal the *dialogical* character of science, by pointing to the connection between linguistic models, esp. the model of the text, and the hermeneutical comprehension of method. In doing so, I shall refer to three contributions to this theme, represented respectively by Dilthey, Gadamer and Ricoeur.

Hermeneutics as the methodic understanding of recorded expressions in Dilthey's epistemology of history

The first philosophical attempt to take seriously the distinct problem of the methodology of the human studies is that of Wilhelm Dilthey (1831-1911). A cursory glance at his works reveals the originality of his contribution to the debate on methodology, in that he brings about decisive reforms in both epistemology and hermeneutics. In this way he succeeds to give new momentum to the debate on the methodology of science, with definite implications for the question of the status of the human sciences.

Although Dilthey's philosophy culminates in hermeneutical considerations, it does not commence as, or develop into a hermeneutic *philosophy*. According to Heidegger (1983: 398) the wide spectrum of Dilthey's interests may roughly be divided into three main concerns: Firstly the history of ideas, which includes the history of the sciences of man, society and the state, the "great poetry of the imagination", and the relation between the culture of his day and philosophy. Secondly, he considers the importance for philosophy of a descriptive and analytical psychology in which the "whole fact" of man is to be presented. Lastly, the distinction between the sciences of nature and the human studies in terms of their respective objects and methods, which leads to his main hermeneutical project.

Following these inquiries, and what concerns our theme, is Dilthey's attempt to design an *epistemology of history* in terms of the notion of *understanding*. This project is a philosophical response to the main cultural achievement of the time, referred to by Ricoeur (1982: 48) as "the creation of history as a science of the first order". Dilthey cherished the hope that through a methodology of their own, the humanities would be able to share in the *emancipation* from classical metaphysics, enjoyed by the natural sciences since the sixteenth century.

In his *epistemology* Dilthey initially adheres to the Kantian notion of knowledge, in which the laws and forms of thought are viewed to contain the conditions of knowledge, and which also provide the ultimate justification of all scientific statements. But, since the judgments of pure (= scientific) reason only apply to knowledge of the physical and mathematical worlds, the question of the possible scientific intelligibility of the *historical* does not arise within the Kantian framework. Kant's well-known negative decision about the possibility of the scientific investigation of those experiences that have a bearing on the meaning of human existence stems from the intention to restrict the limits of knowledge in order to make room for faith. Consequently, historical knowledge has no claim to *rationality* (1978: 655).[1] At best it deserves a place in the realm of metaphysics. However, the unfortunate implication of Kant's viewpoint is that the individual and unique existence of man remains a purely subjective, if not irrational, matter.

Dilthey aims to overcome the dualism of Reason and Life inherent in Kant's thinking by showing that pure reason is itself based on the experience of Life. He accomplishes the *reform* of traditional epistemology first of all by extending the *scope* of epistemological criticism to historical knowledge. In the second place, the reform is accomplished by the incorporation of the type of *empirical explanation* of the natural sciences into the epistemology of history, although in a revised form - via the concept of *understanding*. His leading question is simply, "How can the understanding of the unique be made objective?" Or, to put it in another way, "How is historical knowledge possible?" The project of a *Critique of historical reason* - analogous to Kant's famous *Critique of pure reason* - is thus an attempt to determine the necessary conditions for an empirical science of human history.

In his conception of the *historical*, Dilthey is profoundly influenced by both the speculative idealism of history and the positivistic science of history. The former teaches him that there is "reason in history", although its apriori constructions lack generally valid instances of empirical control. From the latter he learns the necessity of rigorous, empirical and scientific discipline. Its limitations are that it suffers from a narrow conception of *experience*, and also fails to recognize the unique *human* nature of historical phenomena, as distinct from natural phenomena - Rossouw (1983: 32-33).[2]

Accepting that historical knowledge is possible, Dilthey defends his position on the basis of the following arguments. Firstly, he holds that there are fundamental differences between the physical and the mental worlds, i.e. between nature and the mind-constructed world. Whereas there is generally, i.e. in science, a strict separation between the physical and the mental, these are *vitally linked in mankind* (1976: 170). What precedes all knowledge, be it of nature or mankind, is *experience*. Experience, though individual and bound to unique situations, occurs "in a context which, in the midst of change, remains the same throughout life. On the basis of this context, what I have called earlier the acquired structure of mental life develops. It includes our ideas, valuations and purposes and exists as a link between them" (1976: 170).

As a second argument Dilthey holds that the phenomenon of a "structured acquisition", also referred to as the fundamental *interconnection* of life, is the key to the critique of historical knowledge. Knowledge of the unique is possible, not only for the reason of the mind-constructed world shared by individuals. First and foremost it is because life, and the experience of life, produces *meaningful forms* and externalises itself in *stable configurations*. Although the expressions of the mind-constructed world are formed in the sphere of fluctuating experiences which seem to be opposed to natural regularity, mental life is fixed in structured totalities. Through these stable forms, Ricoeur (1982: 50) says of Dilthey's conception, unique individuals are able to understand one another.

For Dilthey, the concept of *understanding* represents the inverse of the process of production, e.g. the expression of an individual genius through the production of a work of art. By understanding, Dilthey means "the process in which we use expressions given to the senses to gain knowledge of mental life". It is thus simply the *re-living* and *reproduction* of the original, creative mental processes of another individual. Interpretation is a special act of understanding in that it pertains to a more limited range of productions, namely those of works fixed in writing. Hence, interpretation is the "methodical understanding of recorded expressions". The methodology of interpretation is hermeneutics. The ultimate aim of all hermeneutics is the psychological understanding of the other mind, which is the psyche of the author in the case of text-interpretation.

For Dilthey, hermeneutics in its classical form as exegetical practice, is considered to be unable to provide the theoretical foundations of the human studies - Bleicher (1980: 19). Hence, in addition to the reform of epistemology, Dilthey brings about a reform of traditional hermeneutics. The latter reform is accomplished by completing the movement that was initiated by Schleiermacher. It involves the incorporation of all regional hermeneutics into a general hermeneutics, which Ricoeur calls a tendency of "*deregionalization*" (1976: 247-263).

In his famous essay on the historical development of hermeneutics,[3] Dilthey shows how the long and respectable history of hermeneutics as experimental science, complemented by the conflict of interpretations, gave rise to the quest for a *unified methodology*, i.e. an objective system of principles in terms of which valid interpretations could be carried out. The need to justify the *rules* of interpretation led Dilthey to the modern, epistemological notion of hermeneutics, namely as "the methodology of the interpretation of written records" (1976: 249). Today this formulation is still widely held, although considerable changes to the scope and task of hermeneutical reflection have been effected.

Schleiermacher's development of a *general* theory of interpretation involved venturing behind the edifice of rules of various interpretive procedures, and analysing the act of understanding as such. For him, not only particular exclusive texts are in need of interpretation, but every act of exegesis presupposes understanding of its author. In fact, all forms of written and spoken communication are potentially subject to the ever present possibility of being misunderstood. Of primary importance is that understanding is the basic procedure according to which the meaningful expression of a foreign subject is comprehended. Dilthey adheres to this *psychological* notion of understanding, and opposes it to explanation. In the latter, man only grasps what is foreign to and distinct from himself. In understanding, the original production of meaning and the signs man offers of his inner existence can be reproduced, because man grasps man and life grasps life. Thus interpretive psychology, the science of the individual genius acting in society and history, is the foundation for the human sciences.

Dilthey sees the task of hermeneutics, and indeed his own, in a twofold way: Firstly, to develop a general concept of understanding as it becomes interpretation in the confrontation with literary products. Secondly, "to counteract the constant irruption of romantic whim and skeptical subjectivity into the realm of history by laying the historical foundations of valid interpretation on which all certainty in history rests. Absorbed into the context of the epistemology, logic and methodology of the human studies, the theory of interpretation becomes a vital link between philosophy and the historical disciplines, an essential part of the foundations of the study of man" (1976: 260).

To conclude, it might be said that for Dilthey every human science, i.e. every modality of the knowledge of man which implies historical relations, is essentialy a hermeneutical science. This assumption holds inasmuch as it is accepted that historical relations are rendered meaningful on the basis of the primordial capacity to transpose oneself into the mental life of others.

Understanding as dialogue in Gadamer's ontology of the hermeneutical experience

The second philosopher that I would like to refer to, is Hans-Georg Gadamer (1900-), a contemporary German philosopher. He takes up the challenge posed by the debate on the status of the human sciences in terms of the core experience presupposed by these sciences, and which, according to Ricoeur's interpretation (1982: 60), constitutes the "scandal" of the modern consciousness, namely that of *alienating distanciation*. The scandal is constituted by the fact that the claim to knowledge of an object, which means to make the foreign familiar, or to become contemporaneous with the past, is from the start frustrated by the alienating nature of the methodic comprehension of that object (1983: 12-17).[4]

Two of Gadamer's main modern influences are Hegel and Heidegger. He follows Heidegger (1889-1976) in the radical adjustment of the entire hermeneutical problematic by attempting to dig beneath the epistemological surface of hermeneutics and to uncover its properly *ontological conditions.* Hegel is followed in his opposition to the subjectivistic notion of the immediacy of experience - in contrast to the *mediational* and *dialogical* character of human experience. In his revision of Hegel's objectivistic theory of consciousness, Gadamer is closer to the Nietzsche-Marx-Freud hermeneutics of suspicion in which the privatized and idealist notion of consciousness is replaced by a tendency towards a *critical distance* from the possible distortions of meaningful forms.[5]

Gadamer's magnum opus, translated as *Truth and method* (1985), takes as its central theme the analysis of the hermeneutical experience in the comparative dimensions of ontology and epistemology. However, he moves beyond Heidegger by rethinking Dilthey's problem in the light of Heidegger's fundamental ontology of understanding. This means that he reinstates the epistemological question of a critique of understanding as a possible mode of knowledge within the framework of a fundamental (ontological) hermeneutics. In the view of Bleicher (1980: 108), Gadamer's philosophy "completes the existential-ontological theory of understanding" and also provides "the foundation for its supersession" through the "*linguisticality* of understanding."

The title *Truth and method* already suggests the confrontation that is to follow between Heidegger's ontological concept of truth and Dilthey's epistemological concept of method. The same tension exists between the primordial relation of belonging as the ontological condition of the experience of the historical, and *alienating distanciation*, which makes possible the methodological objectification of the scientific procedure. Gadamer shows that the ontological participation in the truth as the disclosure of Being effectively excludes the methodological practice of distanciation, which in turn, expresses the destruction of the ontological relation.

Although Heidegger does not undertake the return route to epistemology himself, his fundamental ontology provides the turning point. He initially overcomes the Diltheyan aporia of the ceaseless struggle between rival methodologies through the subordination of epistemology to ontology. However, it does not really resolve the problem, but merely displaces it elsewhere, and thereby aggravates it. Ricoeur comments, "It is no longer between two modalities of knowing *within* epistemology, but it is *between* ontology and epistemology taken as a whole" (1982: 59). The crucial question is how to effectuate the return from the consciousness of belonging to the epistemological problem of the status of the humanities, which is precisely what Gadamer aims to do.

In the light of the above problem, Gadamer pursues the tension between "methodological alienation" and "the experience of truthful belonging" as the two fundamental concepts in the rivalry between epistemology and ontology. In his main work, he traces this tension in the hermeneutical experience through three spheres, namely the aesthetical, the historical and the sphere of language. The latter as it were "cuts across" the first two. Of crucial importance for Gadamer is the *universality* of the hermeneutical experience, as shown in the type of consciousness that corresponds to each sphere.

The universality of the hermeneutical experience can firstly be seen through the fact that the question of language, or better: the mutual relation of thought and *language*, occupies a pivotal position in the philosophical reflection of our modern age.[6] The immense interest in language reveals the attempt to respond to one of the most basic questions of everyday life, which concerns the way in which man is affected by the existence of modern science in its multitude of forms. The *living dialogue* of everyday existence is generated by "the unassailable and anonymous authority that confronts us in the pronouncements of science" (loc. cit.). Consequently, Gadamer holds that the task of modern philosophy is to *mediate* between the scientific employment of man's cognitive and constructive capacities - especially the world of technology and functionality with its corresponding aberrations of technologism and functionalism - and the *totality* of our experience of life. This tension is nowhere better seen than in the instrumental treatment of language, e.g. in the incessant attempt to exercise control over the structure of the texts of our living culture by objective techniques. However, such "use" of language is in the first place made possible by the preceding tradition of the great dialogues and documents of mankind to which we already belong.

The other experiences in which the hermeneutical tension between alienation and belonging is evident, are the aesthetical and historical. Firstly, the original and unquestioned authority of a work of art is lost through the critical exercise of judgement thereof as an aesthetic object. However, the methodic distanciation peculiar to the *aesthetic consciousness* is always secondary to the immediate truth-claim that proceeds from the work of art itself, and by virtue of which it is rendered possible.

The second mode of the experience of alienation is the *historical consciousness*. This consciousness expresses the unique and methodic art, practised in the human and social sciences, of taking up a position of critical distance from historical events. The past is then carefully reconstructed into an historical object with a particular arrangement. Gadamer has no problem with the systematic elimination of prejudices which is of necessity part of the task of the scientist. But the question is whether the methodological objectification of the past does, or indeed can, fulfil the task of understanding the past and what it says to us in the present. Concerning the truth-claim of the past on the present and the future, one may also ask whether there indeed exists such a huge gap between past and present, as Dilthey suggested. This is witnessed when a historical account of events is traced back to its author with her unique perspectives, interests and style. The full reality of the historical experience does not find adequate expression in the "mastery of the historical method". The application of these methods of dealing with the past presupposes the consciousness of being carried by historical traditions.

The tension between truth and method as witnessed in the different spheres of modern consciousness can be illuminated by contrasting it to another, more comprehensive experience, namely that of the *hermeneutical consciousness.* The experience of this consciousness is more fundamental than the mere methodic control one is able to exercise over whatever is alien and leads to misunderstanding, as was the aim of Romanticist hermeneutics proceeding from Schleiermacher and Dilthey. Gadamer suggests that the experience of misunderstanding presupposes a more fundamental experience, namely that of a "deep common accord". It is only because we already understand that we seek to eliminate the occasional misunderstanding. Consequently, the task of hermeneutics cannot be restricted to the technique of avoiding misunderstandings. In every sphere of the experience, misunderstandings fulfil the positive role of prejudices that "constitute the initial directedness of our whole ability to experience".[7]

An important criticism Gadamer (*op. cit.*: 99-100) has of Dilthey's hermeneutics is that there is no recognition of the older tradition of practical philosophy in his hermeneutics.[8] In the latter is incorporated a moment of self-criticism of one's way of life. Dilthey assumed uncritically the modern illusion of explicit, reflective self-consciousness as the unshakable foundation of all certitude. The universal significance of hermeneutics as practical philosophy, for which Nietzsche was an advocate, only became apparent in our time when entire cultures were threatened by radical doubt and critique.

To summarize: Gadamer's reconsideration of the dialogical nature of understanding sheds new light on the epistemological debate concerning scientific methods. It shows that science always pressuposes the interplay of questions and answers that precedes and conditions every attempt to formulate an assertion, a rule and a theory.

The dialectic of understanding and explanation in Ricoeur's hermeneutics of the text

Paul Ricoeur (1913-) is a contemporary French philosopher who has devoted much of his attention to the problems encountered in the historical development of philosophical hermeneutics. Although not a hermeneutical philosopher from the start, his thinking undergoes decisive hermeneutical turns, which eventually brings him to reflect on some of the central themes of interest for present-day hermeneutic philosophers.

Ricoeur's own thought develops into what may be termed a "hermeneutic phenomenology". From the hermeneutical tradition, with its recent existential and ontological bearings, he obtains insight into the *content* of reflection, so that his reflection focuses on the fundamental questions about the meaning of being. And from phenomenology he gains insight about the *form* of reflection: namely the need for methodological rigour, certainty and necessity. His approach can also be called a "Hermeneutics of Reconciliation" and even a "Hermeneutics of Restoration" - see Spiegelberg (1984: 589). In general this stems from Ricoeur's sustained attempts to *mediate* between opposing standpoints, doing it in such a way as to incorporate what is worthy of each perspective into a higher *dialectical unity*.

Although Ricoeur's earlier preoccupations are with Existential Phenomenology, the questions he raises evolves into a philosophical concern in its own right. His interest in the problem of human *willing*[9] develops from a phenomenological analysis of the intentional structures of willing to the reconsideration of the fundamental finitude of man in the experience of human fallibility. Finally it moves to a hermeneutic discussion of the language of avowal, or better, of the symbolic structure of the *language* that is used in the expression of the will's confrontation with the power of evil. His main conclusion from these studies is that the phenomenological inquiry into the problem of the will, with which he started, does not account for the deeper, hidden experiences that are reflected through symbolic expressions and mythical structures. Therefore, philosophical reflection should complement the methodic discipline of phenomenological analysis with the insights of the interpretation of symbols.

Ricoeur's responses to the residues left by the inquiries into the will are twofold.[10] First, Ricoeur reconsiders the hermeneutical problem of interpretation, in so far as it is recognised that philosophical reflection has to undertake the necessary detour through the interpretation of symbols. In the search for a general hermeneutical theory of interpretation, Ricoeur not only finds that such a theory does not exist, but also that there is a "conflict of interpretations" (1974) between rival hermeneutics. He identifies at least two distinctive contemporary approaches to the interpretation of meaningful forms, namely Reductive and Recollective Hermeneutics. The first tendency refuses to take the meaning of a phenomenon for granted and reduces it to a secondary, hidden meaning. Examples of this kind of interpretation, which Ricoeur calls the hermeneutics of suspicion, are Marx, Freud and Nietzsche (1978: 213-219). The second tendency aims to recollect the original meaning of a phenomenon with the view of reinterpreting its meaning for the present. People like the theologian Bultmann, who stems from Existential Phenomenology, adheres to this approach. Ricoeur's attempt to mediate between the two positions is motivated by the assumption that the conflict between rival interpretations can be traced back to the typical symbolic structure of the language of avowal, as well as to the anthropological conflict in the heart of man.

The second important turn in his philosophy occurs as Ricoeur extends his concept of *language* beyond the confines of symbolic language to incorporate a more *scientific* account of the phenomenon of language. On the one hand he reviews his interest in phenomenology, especially the phenomenological analysis of *meaning*, and strengthens it by the insights gained from the work of Anglo-Saxon philosophers of language on the analysis of speech acts and linguistic performance. On the other hand he enters into a debate with Structuralism on the analysis of the structure of mythical expressions. For Ricoeur this debate results in the critical reconsideration of the philosophical implications of the structural method, as well as the ideological assumptions of structuralism as philosophy.

The implications of Ricoeur's work for the development of the hermeneutical problem, and especially for the problem of the status of the human sciences, can now be investigated.

To begin with, Ricoeur draws together the different philosophical tendencies towards the hermeneutical problem in his own thought. As he receives and perceives it, the state of the hermeneutical problem is that it is locked up in two aporias. First, as a consequence of the deregionalisation of rival hermeneutics into a general epistemology of interpretation, hermeneutics is caught up in a conflict of methods: either it accepts "explanation" - the basic mode of knowing in the natural sciences - for the humanities, or it attempts to develop "understanding" as the unique mode of knowing for the distinct, but equally objective, human sciences. Either way, it is uncritically assumed that the methods of the natural sciences constitute the only acceptable model of scientific intelligibility. Secondly, the ontological reorientation of epistemological hermeneutics redeems understanding from its epistemological stranglehold and reinstates it as the primary ontological phenomenon. However, the ontology of understanding opposes the epistemological concern for method and the primordial participation of the knowing subject in the light of the truth-claim of the object. Thus it follows that Dilthey's problem is not resolved, but merely displaced. A new conflict is generated in which the clash is not between rival methods, but where the alternative is *either* ontology *or* epistemology, *either* truth *or* method.

Ricoeur's attempt to avoid the above pitfalls in his critical rethinking of the hermeneutical experience focuses on the dialectical mediation between the respective oppositions of explanation and understanding, and of method and truth. He states his own problem in terms of the question, "How is it possible to introduce a critical instance into a consciousness of belonging which is precisely defined by the rejection of distanciation?" (1982: 61). And in another essay, "Existence and Hermeneutics", he asks, "What happens to an epistemology of interpretation, born of a reflection on exegesis, on the method of history, on psychoanalysis, on the phenomenology of religion, etc., when it is touched, animated, and, as we may say, inspired by an ontology of understanding?" - quoted in Bleicher (1980: 238). The answer to these questions is to be found in the dialectical mediation between the opposition of belonging and distanciation. Not surprisingly, Ricoeur calls his own project, in contrast to the movements of deregionalisation and radicalisation, one of "reregionalisation." This means that he aims to extend the scope of hermeneutical reflection from a dispute within philosophy to a dialogue with the individual sciences, especially the sciences concerned with text-interpretation.

A starting-point for Ricoeur is Gadamer's concept of "*historical consciousness*", which assumes both the repudiation of a one-sided methodological consciousness, and the positive acceptance of an element of distance in the experience of understanding. For Dilthey, the historical consciousness refers to the awareness of the relativity of all historical phenomena. For Gadamer, this awareness pertains not so much to historical method of knowing as such, but rather to the reflective consciousness of the methodology. It is the consciousness of the history of effects, of being exposed to history and its action and effects. History's action upon us cannot be fully objectified, because our consciousness is part of the historical phenomenon itself.

Another, and most important possibility to mediate dialectically between method and truth Ricoeur finds in Gadamer's assumption of the fundamental *linguisticality* of all understanding, to which the entire third part of his major work is devoted. With that, he claims (1982: 62), Gadamer refers to the fact that one's belonging to a tradition or traditions of necessity passes through "the interpretation of signs, works and texts in which cultural heritages are inscribed and offer themselves to be deciphered".

The long route that Ricoeur proposes to follow - the counterpart of Heidegger's short cut through a direct ontology - prompts him to take as his leading question, "Is it not once again *within language* itself that we must seek the indication that understanding is a mode of being?" - in Bleicher (op. cit.: 243). A reflection on the objectification and interpretation of *meaning* in language provides for Ricoeur the possibility to mediate between the "ontology of understanding" and the "epistemology of interpretation".

Ricoeur raises the question whether Gadamer's theory of the linguisticality ("Sprachlichkeit") of all understanding does not presuppose a concept of *writing*, or "productive inscription" ("Schriftlichkeit"), where the direct mediation by the spoken word becomes the *distanced* mediation through the text. "It is first of all and always in language that all ontic or ontological understanding arrives at its expression" (Ibid: 245). Writing is one of the fundamental forms of expression in all communication, and as such deserves special attention.

In his work, *Interpretation theory* (1976), Ricoeur departs from the Saussurean dichotomy (see De Saussure - 1974) between language as the objective system of linguistic signs ("langue") on the basis of which a particular speaker produces a particular message, and language as the actual event of saying ("parole"). The positive consequence of this dichotomy is that linguistics could make excellent progress by bracketing the message for the sake of the code, and the event for the sake of the system. The negative consequence is that it severs the natural relation between the objectively construed structure of language and the personal act of speaking and listening. For the objective system of signs, language as discourse disappears.

Ricoeur's response to this dichotomy is that he develops a theory of language as discourse that mediates dialectically between both moments of the objectification of language. In order to account for this mediation, Ricoeur firstly constructs the idea of the dialectic of *event* and *meaning* in discourse. The event of speaking is not a mere historical and subjective act, but it already involves a certain objectivity in that the subjective intention of the speaker (utterer's meaning) is surpassed by the *meaning* of what is said (utterance meaning). Not only does this distinction provide a new set of criteria to differentiate between discourse and linguistic codes, but it provides a departure from the psychologistic reduction of meaning to subjective intention. The concept of meaning is derived from Husserl's phenomenology, as well as from Frege's distinction between sense and meaning (1960: 61) in which it is established that it is not so much mental life as such that is grasped, as Dilthey would have it, but the identifiable meanings intended and expressed by the mental life. Ricoeur formulates the implication of the theory of language as discourse for the hermeneutical problem as: "If all discourse is actualized as an event, all discourse is understood as meaning" (1976: 12).

The last part of Ricoeur's hermeneutics that I would like to refer to is his hermeneutics of the *text*. This theory is not separate from the concept of language as discourse, but develops from it. Of importance for the 'aporias' of the hermeneutical debate is Ricoeur's distinction between the speaking-hearing relation and the writing-reading relation. His argument is that, although both speaking and writing are events of speech, the latter is not a mere extension of the former. Different conditions prevail in each situation. The inscription of discourse is "the transcription of the world, and transcription is not a reduplication, but metamorphosis" (op. cit.)[11].

With the reading of a text, and the understanding of the meaning of the message, writing becomes a hermeneutical problem. Adhering to Dilthey's restriction of hermeneutics to the methodic interpretation of *recorded* expressions of life, Ricoeur establishes the *paradigm* of the text[12] in terms of the basic criteria of the "autonomy of the text": (a) The fixation of meaning in the text gives greater priority to the meaning of what is said than to the psychological intentions of the author. Thus the text's 'career' escapes the finite intentions of the author. (b) The world of the text surpasses the ostensive references of spoken discourse and frees the meaning of the text from the limitations of a particular situation. As such, the *structural explanation* of a text represents a moment of validation in terms of objective rules, which can be dialectically linked to the comprehension of the meaning of the text as a whole. (c) The text, by virtue of being fixed, achieves in principle a universal reading public, the intersubjectivity of which allows for a much more objective interpretation.

The relation reading-writing enables the reader to distinguish between the psychological intentions of the author and the *matter of the text*, and so opens the way for the dialectical mediation between understanding and explanation in interpretation. Far from being a mere quantitative phenomenon of a spatio-temporal separation, distanciation refers to the dynamic counterpart of the need and effort to overcome estrangement. Especially in interpretation, the distanciation is a *productive* moment in the revelation of a new text of reality.

Lastly,[13] I want to refer to Ricoeur's argument that the paradigm of the text, and of the methodic interpretation of the text that includes both moments of explanation and understanding, can be extended to the methodic comprehension of "meaningful action." In other words, he suggests that the theory of interpretation can be extended to the social sciences. In the same way that the interlocutionary situation is overcome in writing through the fixation of meaning, the fluid character of "interaction" is overcome when action is treated like a text. Meaningful action also shares the other constitutive features of a text, esp. autonomy and openness. Like a text, human action is an open work, it opens up new references and invites fresh interpretations. And, as a text is detached from its author and its dialogical situation, an action may be detached from the agent(s) and its situation, and objectified in a "record" or "register" that implies responsibility and accountability.

To conclude the discussion on Ricoeur, I would like to quote him in dialogue with Richard Kearney (1986: 45), in which he distinguishes between structuralist analysis and phenomenological hermeneutics:

> Whereas the former is concerned with the immanent arrangement of texts and textual codes, hermeneutics looks for the 'meaning' produced by these codes. It is my conviction that the decisive feature of hermeneutics is the capacity of world-disclosure yielded by texts. Hermeneutics is not confined to the objective structural analysis of texts nor to the subjective existential analysis of the authors of texts; its primary concern is with the worlds which these authors and texts open up. It is by an understanding of the worlds, actual and possible, opened by language that we may arrive at a better understanding of ourselves.

Finally, I would like to make the following observations:

1. It would seem that the debate on the methodology of science is not an esoteric and intellectualized monologue. Rather, it is a response to a situation in which the scientist is acutely aware of the limitations of methodic practice without methodological considerations.
2. The hermeneutic dialogue focuses mainly on the systematic considerations assumed by the individual sciences. It cannot and should not wish to replace inquiries in the particular regions of the scientific enterprise. However, it acts as a kind of mediator between opposing approaches in pointing out the "common accord" that perhaps already exists between them.
3. Only to the extent that every individual science determines for itself in what sense it is a hermeneutical science, will the debate on methodology, and indeed the dialogical character of science, come to their own.
4. Far from being exhausted by the preceding remarks on the hermeneutics of the text, the rhetorical approach to the text, e.g. the text as metaphor, seems to mark only the beginning of yet a new "text of reality." With Ricoeur (1978: 9) one may ask: Is not the history of rhetoric an "ironic tale of diminishing returns"?

Notes

1. For Kant, historical knowledge is merely *cognitio ex datis* and does not, as such, qualify as *rational knowledge*, which is described as *cognitio ex principiis*.
2. Also see W. Dilthey, (1976: 157-158), in which it is argued that in the views of such men as Comte and Mill, the "moral sciences" only share in the emancipation from metaphysics if they rigidly adopt the methods of physics and chemistry, and so form part of the system of nature. According to that system, the nature of things is immutable and accessible to reason, and the fixed human nature is only superficially affected by social and historical factors. The basis of actual codes of law is "natural law" and the rational core of all religions "natural religion". The historical school, with whom Dilthey identifies, rejects these assumptions, arguing that the humanities are not thereby emancipated,

but merely exchanged subjection from metaphysics to natural science. Their basic claim is instead that laws, religions and human nature are products of history and subject to historical change, which naturally calls for a concept of experience and a systematic approach that are more suitable to the subject-matter.

3. Published under the English title, *The development of hermeneutics*. In his introduction, Rickman states Dilthey's main arguments: "Understanding man and society is more like interpreting a text than acquiring knowledge of the physical world by using the methods of physics or chemistry. Therefore the traditional skills of literary critics, jurists, linguists or Biblical scholars must be systematized and transformed into a methodology of the human studies".

4. In the essay, *On the philosophical element in the sciences and the scientific character of philosophy*, Gadamer (loc. cit.) argues that the modern mechanical concepts of theory and practice is so far removed from the original Greek ideal of rationality that "science always has and always will come up against a claim of comprehension in the face of which it must fail - and indeed which it should forgo". And, looking at the effect of the transformation of the Greek "theoria" in modern times, he says that the puzzling thing is that "the rational need for unity has been repeatedly disappointed by the progress of research and to its astonishment it has learned to find its balance in the mids of a manifold of peculiarities that each in themselves possess the particular unified structure peculiar to systems. I think it is symptomatic that systems theory has displaced systematic constructs".

5. Ibid. xxi. (F.G. Lawrence in the Translator's Introduction). Despite their acknowledged influence on his thinking, Gadamer is not uncritical of Hegel or Heidegger. He also is involved in serious debates with Habermas, though he would endorse much of the latter's criticism of philosophers like Popper and Albert. Through the "grafting" of the phenomenological method on the hermeneutical problem, as Ricoeur calls is, Gadamer is also much closer to the Recollective Hermeneutics of a Bultmann and of Ricoeur himself. Another influence on his thinking are the masters of Greek thinking, which has led Gadamer to the quest to look for "the Plato in Aristotle and the Aristotle in Plato".

6. Gadamer, in the essay, *The universality of the hermeneutical problem* (1977: 3), holds that, "Language is the fundamental mode of operation of our being-in-the-world and the all-embracing form of the constitution of the world".

7. (Op. cit.: 8). On the level of the aesthetic consciousness, the hermeneutical experience is displayed through the same tension: "There are no purely formal criteria that can claim to judge and sanction the formative level simply on the basis of its artistic virtuosity. Rather, our sensitive-spiritual existence is an aesthetic resonance chamber that resonates with the voices that are constantly reaching us, preceding all explicit aesthetic judgment".

8. See Ricoeur's trilogy on the Phenomenology of the Will: *Freedom and Nature: The voluntary and the involuntary* (1966), *Fallible Man* (1965) and *The symbolism of evil* (1967).

9. Demonstrated in my M.A. Thesis (1985: 19, 134) on Ricoeur's hermeneutics.

10. Extending Heidegger's concept of the world, Ricoeur says, "Thanks to writing, man and only man has a world and not just a situation" (op. cit.: 36).

11. Ricoeur (1982). See the essays, *What is a Text? Explanation and understanding* and *The model of the text: Meaning action considered as a text.*

12. Important parts of Ricoeur's reorientation of the hermeneutical problem are omitted, esp. the references to his theory of metaphor. The reason for this is simply that his theory of the creation of meaning through metaphor (1978) warrants a discussion of its own.

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9

Why do we need interpretation?

Leopold Peeters

My question concerning the pertinence of an interpretation is prompted by two commonly held presuppositions which I should like to explicate and subject to questioning themselves. First of all, it is generally accepted that science should have practical application. Secondly, interpretation in general and text interpretation in particular are dismissed as subjective in the superficial and pejorative sense of the word. It is my intention to show that the rejection of interpretation is based upon a false conception of its nature and that it can not be put aside as irrelevant. I should subsequently like to show what the usefulness of interpretation can be.

For most, the term "interpretation" is spontaneously and inseparably linked to hermeneutics, the latter being usually understood as the art of interpreting texts. It is also widely assumed that the hermeneutical activity is something of the past, since it supposes that truth and knowledge originate in texts in which some or other revelation about reality and its meaning is contained. Originally hermeneutics as the art of interpretation - as the word still shows in its derivation from the name of Hermes, messenger of the gods and their mediator - concerns the correct understanding of a revelation formulated in the word, spoken or written, with the result that knowledge would be of a lingual nature. This situation finds its manifestation in the Greek notion of logos which designates the order of being as it is revealed in language. But our situation in modern times seems to be totally different: what we know for sure about reality is no longer derived from texts nor from a tradition as it is preserved in poems and treatises, but from observation and experimentation which find their expression, as measurable and applicable information about reality, natural or human, not verbally but mathematically. Mathematics in turn makes it possible to calculate and arrive at indubitable results which at the same time allow planned action. Scientific knowledge is useful and this constitutes its justification. Science is the foundation of technology which allows man to intervene in the world and in the order of things (which words do not, except in fairy

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tales and in myths). Since human intelligence has defined the aspects under which the world becomes objective, it can, with a great degree of success, transform that which it has determined and in some way created by definition. Technology enables one to remedy deficiences and to correct errors, produce goods and services, cure illnesses and combat plagues, store and create energy. Scientific knowledge gives real power in the form of energy which permits production. Therefore it is a good investment. It is generally supposed that man now has the power to plan the world according to his own needs and dreams. Even if it is still feasible to question the wisdom of transforming the world into a man made place, even if it is uncertain whether we have properly understood the essence of technology and can foresee its consequences, the fact remains that in the higher institutes of learning the humanities are on the defensive and in need of justification. This need however is not a recent development: since the moment the natural sciences began to prove their effectiveness, the human sciences have been forced to rethink their own nature and foundation. First of all philosophy sought to establish the conditions of possibility for objective knowledge as founded a priori in the human mind, with the consequence that philosophy more and more became epistemology. At the same time hermeneutics was resuscitated in an effort to confer on the historical sciences the same objectivity which made the natural sciences so reliable.

Modern thinking is marked thus by the question about the nature of human knowledge and its possible unity which has so evidently broken up and been divided between objective knowledge which separates and alienates the subject on the one hand, and on the other, the belonging of that same subject to a tradition, in which the subject learns to know himself and finds a dwelling: in this tradition, space and time become human and the world becomes a place we can live in. Science on the other hand situates the subject at a unbridgeable distance from reality which it can only represent and reproduce. These abstract representations allow him to put in the place of living reality, a dead artificial duplicate, a double.

The preceding remarks intend to situate our effort in this paper to try and understand the true nature of interpretation and to show its relevance even in this our technological age. To conclude these introductory remarks two observations must still be made to prepare the subsequent reflection. As far as science is concerned: epistemology has established that science in order to be scientific must be falsifiable. It can not be realistically believed that one day man will know everything about everything. Science thus becomes a promethean endeavour and its lack or want is not because of the provisional state of its art, but because of the infinite scope of its task. Secondly it is evident that technology has not yet succeeded in transforming our world into the paradise promised by dialectical materialism, not even in those parts of the world where that so called science has now had 80 years to prove itself. Here again it is not the fact of the provisional state of the art, but the essential finite nature of things human which makes the dream of total control of reality impossible to realise. This finitude is clearly linked to the irrefutable fact that wherever technical progress is persued, it almost always becomes an end in itself, and man becomes subordinated to it. When a researcher thinks a technical intervention or experiment is possible, he is at the same time convinced that it must be tried, whatever the consequences and without stopping at the ethical questions that the experiment might pose. It is certainly not possible nor advisable to turn the clock back and to undo what science and technology have done, that would amount to undoing the whole of the Western tradition since it is abundantly clear that science and technology are a direct continuation of the original efforts made by mankind to understand its own situation in the universe. It is of the utmost importance however to prevent our technological advancement from destroying human values. And in this respect the human sciences might be of help. It would be as destructive to oppose the natural to the human sciences in rivalry where one would try to obliterate the other: they have in any case a common origin. And there is still one element which constitutes that unity and that is language. Even for the most formalized of theories, language has been proven to be indispensable (Wittgenstein). And so it still holds what Ricoeur, whose work we will take as our starting point in this discussion, identifies as the most important and urgent tast for modern philosophy or thinking: to determine the ontological status of language. I think that a reflection on the nature of interpretation even of the most impertinent and innocent of texts, poetry, can help us to advance towards that goal. But advance might not be the correct word in this context: it would rather help us to clarify the position of man as an understanding being.[1]

Ricoeur,[2] not unlike Hermes in that sense, is essentially a mediator. The opposition between natural and human sciences, which has been the main concern of epistemological thinking since the end of the 19th century, finds its methodological expression in the antinomy between understanding and explanation. Basically the act of understanding takes place in a homogeneous field in which understander and thing or reality understood are of the same nature: the subject understands its own position in reality. Explaining on the other hand entails a relationship between heterogeneous realms of reality: the human mind which explains on the one hand, and objective reality on the other. Understanding would essentially be based on intuition and self reflection whereas explanation posits the thing explained outside the zone where the explanation is given. Understanding then would pertain more specifically to what the mind has produced and created, which we could designate by the word culture; explanation concerns what nature presents to the human mind. In modern times and especially since Descartes these two realms are held to be irreconcilable. An easy way out of this dualism would be to obliterate all spiritual reality as materialism proposes to do, or to consider all matter as a manifestation of the spirit, thereby admitting reality only in as far as it exists for a selfconscious subject. But both attitudes are clearly reductionistic. Ricoeur tries to find a reality which would be at the same time an object (and thus would fall under the competence of an explanatory effort) and a manifestation of the human spirit (and would therefore address itself to the intuitive and self reflective power of the mind).

He believes to have found such a reality in the form of the text. The text according to Ricoeur is an example of a relation (with the past, the other or the world) via distanciation, it is a mediating object par excellence, a symbol in the original sense of the word where two halves reform. Needless to say, the discipline in the human sciences which has a special competence as far as texts are concerned, is hermeneutics, not only in the original sense where the texts taken into consideration would have to be of a religious nature, but in its generalized meaning of the whole of methodological prescriptions to be followed when a text has to be understood. But in the revival of hermeneutics, at the beginning of the 19th century, especially in the work of Schleiermacher, the reflection on the method of understanding was turned into the more general question of nature of understanding itself. The consequences of this change in direction transformed the concept of understanding and gave it a more general scope to the point that in the philosophy of Heidegger understanding becomes the ontological relationship between man and Being. And the latter consequently becomes open to man as language. Hence the enormous importance of language as it manifests itself in all forms of texts and this manifestation occurs in time and is essentially historical. In Heidegger's vision poems are texts in which true language comes to be spoken so that a world comes into being. To read a poem (in the original sense of the Greek word *legein*) or a philosophical text is the same as understanding being.

But this ontological reading still lacks an objective basis in the eyes at least of scientific knowledge since the understanding transforms the text by approaching it from a historically situated point of view, thereby relativising the results of the personal reading to the point of ruining its relevance. Heidegger's reading in particular has been criticised for lack of scientific or objective validity since it consists in an act of listening submission, not to the letter of the text, but to that which speaks through the text; it consists in trying to uncover what remains unsaid in the text by engaging in a dialogue with it. This attitude could lead to a certain arbitrariness on the part of the reader who claims to unearth what he decides to be worthy of thought and meditation. This attitude is totally opposed to that of a Wittgenstein (*Philosophical investigations,* 464) who sees it as the task of the reader to distinguish in the text between sense and nonsense, to reject as nonsense what is not immediately selfevident. It is also different from the same philosopher's later theory of word games wherein speakers are locked into commonly and conventionally accepted ways of speaking or writing. But whatever the criticism levelled at Heidegger's reading, it must be stated that it does not intend to let subjectivity loose on a text, nor does it allow a kind of interpretative delirium. It is in any case doubtful whether the subject could be totally eliminated from a cognitive experience: in science too explanations and results are based on conventions ruling almost supreme within the scientific fraternity. As such, interpretive reading does not intend to promote subjective understanding in the sense that it would be false, unreliable, unrepeatable and uncontrolable. Its intention would rather be to install a dialogue amongst the readers of a given text. In order to establish the objective or controlable aspects of reading, Ricoeur proposes to take into account the methods used by structural linguistics and semiology, which, according to him, would give to the human sciences their true but also specific scientificity. A structural or semiological analysis of the text would confer on a reading an intersubjective controlability and validity. Here in fact lies the problematic nature, according to me at least, of Ricoeur's mediating effort. It is obvious that all theory (linguistic or semiologic) rests upon the possibility of working with well defined systems; however, it is irrefutably clear that natural languages and their realisations in speech or writing are not and can not be considered to be well defined systems. In fact it is doubtful whether reality comprises any well defined systems at all.[3] The only ones possible would be those constituted by the human mind, and these would then have to be put in the place of real reality. These systems are in essence indifferent to time and space. It is obvious however that they are conceived of in a temporaly and locally finite human body. To claim to transcend these limitations is therefore to say the least problematic. Ricoeur nevertheless proposes a dialectical combination of understanding and explanation towards the formulation of an interpretation. Let us first characterise interpretation as Ricoeur sees it. His point of departure is the observation that science is related to the ontological situation of the knowing subject, a subject which itself belongs to reality. Intentionality which unites subject and object is situated inside an encompassing whole out of which the knowing subject can not step. Therefore, knowledge remains limited and finite. Our being-in-the-world never allows us to take the position of an absolute mind and for that reason reference to reality can not be absolute either. We can only think and meditate about reality; and our knowledge will always be selfreflecting, it will be knowledge for us and in order to be, it needs the mediation of interpretation. Interpretation in this context can not be regarded as a methodology but rather as an explication of experience. This explication is necessarily mediated by natural language. Since the latter is essentially and irrevocably polysemic it needs a relationship with a context and also dialogue in order to be correctly understood; dialogue and the context are historically, socially and temporally situated. From these basic aspects of the situation of human knowledge derive some consequences for the interpretation of texts: this interpretation is the making explicit of a being-in-the-world as mediated through a text, the latter being a proposition about how a world comes into being in its relationship to a conscience. The human subject does not possess its own meaning in itself and in total transparency to itself but finds itself necessarily via the world as it is constituted in a tradition. Consequently reading is not the affirmation of my own subjectivity but a response or answer to what is proposed in the text. While reading I cease to be master of myself in so far as I submit to the injunctions of the text before returning to myself. Linked to reading then is a catharsis which constitutes also an enrichment in as far as I accept to "assume" the text. Hereby we rediscover the nature of catharsis as it was conceived in antiquity.[4] Its original function was linked to a socio- religious, cultual and cultural celebration which restored the harmony between man and the universe and man and his fellow human beings. Something analogous could be said of reading. Modern science tries to take up a position outside reality in an endeavour to reach objective knowledge, but this knowledge still remains the result of axioms commonly and willfully accepted by the scientific or human community. The ontological status of the subjective body however does not allow such absolute objective knowledge. It would seem that technology in the form of the computer could one day make this type of excarnation possible by providing almost infinite capacity for storage and manipulation of information.[5] Also telecommunications would seem to make possible instant duplication, hence what I call excarnation of all happenings and events in the world. But this infinite knowledge would not be absolute in so far as it will always come second: it will be relational and therefore relative. Certainly the power to manipulate matter and even fellow human beings will thereby be greatly enhanced. But the manipulator or the tyrant will never be able to coïncide with the manipulated and the oppressed. To be absolute and totally objective, knowledge would have to coïncide with the known; this would only be possible if the latter was entirely produced by the human mind. Since that is not the case one can assume that there will always be a chasm between the known and the knowing: coincidence between the two is only possible in a state of contemplation whereby knowledge forfeits its will to master reality. In provisional conclusion then: interpretation is relevant because it is unavoidably part of all cognitive operations executed by the human mind. What we know about reality is always an interpretation or, epistemologically speaking, a hypothesis which must be falsifiable in order to be truly scientific or objective. Some hypotheses are surely more applicable and usefull than others in so far as they permit concerted and effective action. But the use of knowledge will always be a matter of human decision. It is on this level that we now want to try and establish the usefulness of text interpretation, again by following in

Ricoeur's footsteps. This time the strategy consists in showing that text interpretation could be a paradigm for the human sciences.

Recent work in the field of action theory leads to a working hypothesis: that it is possible to consider a text as action and action as a text. The former possibility has been explored i.a. by Stierle (1981) who discusses the text as action, whereas Ricoeur proposes to interprete action as a text. Basic to all these discussions is the centrality of language: human experience is essentially of a lingual nature. If action can be considered to be a text, then hermeneutics, which traditionnally is the art of interpreting texts, can become a paradigm for the human sciences and text interpretation a paradigm for the understanding of human action. First of all Ricoeur proceeds to define the nature of discourse (speech and text) as against language systems. The speech act is a punctual event situated in space and time, carried out by somebody who refers to himself as the source of speech (auto-referential) but says something about the world which through language becomes symbolic; this is done in a situation where there is participation with the other and also exchange. Language on the other hand would be extra-temporal, subjectless, systemic, and a precondition to communication or circulation of information. But speech in order to be preserved, must be recorded. This recording, written or audio-visual, presents us not only with what is said. What is said does not only coïncide with the content, information, expression, etc. What is said or written includes what is meant to be achieved by writing or saying it and what is in fact achieved. All this becomes availabe in a (more of less) unchangeable form. In the text (document) discourse becomes detached from the intentions of the speaker, the text is taken out of the living horizon of the speaker-writer, detached from the psychology and the intention of the author. This gives rise to a greater sprituality of the text which now has to stand for itself and needs interpretation. The text also refers not to the Umwelt (which can be pointed out) but to the world; in a text we can observe how a world comes into being, it offers a world project; the text is an endeavour to humanize the world. Since the text is spiritual and free from all too concrete situations which are embodied and often alienated, it can become universal.

The next step would be to seek for analogies between the readability of a text and the understandability of meaningful human action (attitude, behaviour). Can behaviour become as "objectified" as speech becomes objectified in a text? Do they have a similar structure? Ricoeur points out that behaviour is like a proposition: it has content, meaning, results, intention and consequences. It becomes independent of the acting person as it is registered and spoken of. It becomes "filed". Its consequential importance is greater than its immediate pertinence and it remains open to different posterior evaluations which change its meaning. Action in other words stays open.

This analogy between text and action then makes text interpretation a paradigm for the human sciences. It is not so much this paradigmatic value of interpretation which interests here. Text interpretation is not only valid and pertinent because of its possible methodological value. In my opinion text interpretation even of poems has a value in itself. Therefore I want to challenge Ricoeur's concept of a text as immaterial and free of limitating situational constraints. Certainly a poem is not only information about experience, it is also the incarnation of the experience which can be shared by the reader. In other words: the poem is not only a carrier of experience but in some way is that experience itself. The poem therefore is not a representation but an original phenomenon. Our analogy, if a poem and its reading is to be usefull in an analogous manner at all, would be between itself and the human body. In order to form this analogy on a sound basis it is necessary to have a proper understanding of what the human body is.[6] I can only give some directions in which the answer is to be sought.

First of all and in its essence the body is subjective, and is originally revealed in a sphere of absolute immanence. Therefore subjectivity can not be impersonal as it was the case in transcendental philosophy. The body is not a simple "transcendental" field which at the end of classical thought dissolves into a pure mirage, into an empty continuity, a simple representation devoid of all content. Subjectivity is not a pure nothingness but a realm or region of concrete being. It is not an empty milieu which floats around in representation, it is not transcendental but a sphere of absolute immanence, life itself. Opposed to this living subjectivity all abstract transcendental notions are dead. That which maintains life is not understanding life or knowing it but life itself. Subjectivity is the density of life which is not a pure form but has a content of its own, situated in the world without being (an integral part) of it.

Nevertheless life goes on outside the transcendent body, as subjectivity which is not a pure spirit enclosed in its own nothingness, incapable of descending into the determination of life. Since subjectivity is life it is the seriousness of existence. From these few remarks about the subjective body we can deduce some basic characteristics of the poem, which could be said to be in some way coextensive to it, to be its continuation in language. But language itself is of a bodily nature.[7]

Since Kant, science has eliminated the existential subject from knowledge: to be objective, knowledge must be impersonal, indifferent and interchangeable and all reflexivity must be eliminated from it. Objective knowledge turns the subject away from its own existence and thereby also from that of the others. I have called this an exercice in excarnation. Scientific discourse is excarnation. On the other hand, and to counter this objectivation, phenomenology has insisted on the transcendental nature of the subject thereby falling into the trap of solipsism: the world would only be in as far as it is for me. I propose to consider the subjective body as the mediating reality. Our body - to speak in terms of Merleau-Ponty's (1964) phenomenology - puts us in a chiasmic relation with the outside world.

Through our body we are part of the living world which we can share with the others. This we do via language. The conception of language as a system of signs permitting communication is reductionistic. Language is ontologically linked to the fact that we are a body. Language is articulated as our body is, when we speak we use the whole of our body. This body was, originally, shared with our mother: having being in her womb we have come to experience rhythm as a meaningful movement. As soon as we were separated from our mother we have to learn about our own body at the same time as we have to learn to use language in order to be with others. Language at its beginning is a bodily response and answer to the surrounding world from which we are separated while staying engulfed in it. How we feel in the state of our personality in the world is manifested in the transitional object, which psychologists like Winnicott (1971) have studied and which they hold as creative of precisely that realm where religion, culture and art find their concrete origin. In that sense a poem would also be some kind of transitional object. Sure enough, language is also impersonal in sofar as its syntactical structures and rules are in some way codified. It could also be said that speech becomes fossilised in language. There is however one element in speech, which is irreducible to the linguistic system. That element is rhythm. Rhythm is the most evident and tangible bodily aspect of speech. In poetry and prose alike, this rhythm is still present in the configuration of the text in its entirety. True interpretation consists in searching for the rhythm of the text. Rhythm is a sensory part of the text, in which the experience it conveys is incarnated. It therefore allows the reader to participate in the way a world came into being in language for the person who wrote the text. Writing a poem therefore is not writing about something but is writing oneself and offering a part of oneself as a kind of sacrifice.[8]

The poem would consequently be a body which can be resuscitated by the act of reading (this I would call interpretation). The reading of the poem allows it to find a new life and, as a result, to become vivifying itself. Almost like those seeds one finds in the desert waiting only for a drop of water to open up and start blooming, a poem would be life at its most improbable: it depends on the patience and charity of a reader in order to come into existence, a reader who will assume it and bring it to fulfilment.[9] Therefore we have to invert our original question. Instead of asking what to do with an interpretation (this question would lead to some or other benefit) we should rather demand what an interpretation does with us. The act of interpretation changes us and brings us back into the realm of solidarity with living matter. Interpreting opens us up to the others which are our responsability. In that sense the poem transforms all of us into poets, appeals to the poet who lives in each of us, the poet who was at work at the time we learned with great tenacity and abandon to know our own body in the world and at the same time learned to understand and speak, and to assume what others said to us.

Notes

1. The ideas expressed in these introductory paragraphs do not originate in any specific book or study but are inspired by the work of thinkers such as Apel, Descombes, Diéguez, Gadamer, Heidegger and Popper; see References for some of their pertinent works in the context of this paper. As far as hermeneutics is concerned, a useful introduction in English is Palmer's book on the subject.
2. Although the whole of Ricoeur's work is relevant to our discussion this paper is mainly concerned with his *Essais d'herméneutique II* in which he reflects on the path of his own thinking.
3. For a discussion of language as a system see Hockett (1975). In this context one can also refer to the work of philosophers and linguists such as Jacques, Jaspers, Lohmann, Hagège, Wandruzska.
4. The concept of "catharsis" is quite complex and poses many problems to its correct interpretation, especially because Aristotle fails to define it in his treatise on poetics. See References for more information.
5. The power of information is one of the most important aspects of what has been called by Lyotard "la condition post-moderne" (1979).
6. For a profound reflexion on the human body we refer to the work of Michel Henry which derserves more attention. His book on the phenomenology of the body has been translated in English and publised by Martinus Nyhoff (The Hague). It will be necessary to come back more systematically and extensively on this French philosopher's work.
7. The biological foundations of language have been studied by anthropologists and ethnologists alike; usefull indications can be found in Lohmann but especially in the work of Leroi-Gourhan and of Marcel Jousse. See References.
8. Montaigne seems to be the first to have made his own person the object of his writing in the sixth chapter of the second book of his *essays* where he states: "It is not my actions that I write; it is me, it is my being".
9. Recently and in a very explicit way the sacrificial value of poetry has been affirmed by Ernst Jünger in his *Autor und Autorschaft* (1984): "The poem is a sacrifice (*ein Opfer*); often this stays hidden, but where it is not brought as a sacrifice, the poem disperses like noise or smoke". The idea of the poem as a body susceptible of being ressuscitated by the reader has been advanced by the French contemporary poet Bonnefoy (see References) or as an extension of living matter by someone like Lorand Gaspar (1978). A most moving way of

meditating on the ressuscitational act of reading can be found in a letter by the French poet Perros (1987) who knew he had terminal cancer but insisted that true poetry had already solved the problem of death by its participation to the whole of the living universe.

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10

Jarvie on rationality and the unity of mankind*

David Sapire

Introduction

Is it possible to believe that science as it has developed mainly in the West reigns supreme among systems intended to provide knowledge of the world, yet not be committed to derogatory views about other cultures and societies? Many anthropologists and others would answer, "No". Orthodoxy held and largely still holds that all such comparisons are invidious and are to be avoided. One way of avoiding them is offered by symbolism, which maintains that traditional systems of magic and the like cannot be compared to science, but only, perhaps, to poems or paintings or pieces of music. Being somehow symbolic rather than scientific, they cannot be inferior or defective science.

Lately, the tide has begun to turn against recourse to approaches like symbolism in anthropology. For instance, in an influential paper entitled *African traditional thought and western science,* Robin Horton has argued that traditional African systems of religious and other belief are at least in part in the same business of *theory* construction as Western science: they do aim to describe, explain, and predict phenomena.[1]

Similarly, Jarvie and Agassi have argued in another influential paper that ritual acts of magic are not essentially symbolic but are rather rational in being explicable in terms of certain goals and certain beliefs about the world. These beliefs, however, are not as rational as scientific ones.[2]

In a recent book, Jarvie clarifies and extends his view.[3] He not only rejects symbolism but argues that science is nothing less than "the supreme embodiment of mankind's rationality" (p. 21). And he mounts a concerted attack on relativism. This attack rounds out his view because relativism has played the same kind of role as symbolism in modern anthropology. As Sperber aptly puts it:

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> In pre-relativist anthropology, Westerners thought of themselves as superior to all other people. Relativism replaced this despicable hierarchical gap by a kind of cognitive apartheid. If we cannot be superior in the same world, let each people live in its own world.[4] And yet, Jarvie maintains, his view is not at all derogatory or disrespectful.

In the main, Jarvie's case seems to me be both timely and well supported. But I also think that it suffers from certain shortcomings which, though serious, can and should be removed. I concentrate here on two in particular. The first and more important of them is an ambiguity in his basic notion of *rationality*. As a result, his claim that science is the supreme embodiment of rationality cannot but amount to a strong (even if inadvertent) slur on many cultures and peoples. The second is his conviction that theses of the basic psychological unity or similarity of all humans are metaphysical; they are not refutable by empirical evidence. He therefore chooses neither to assert, nor to deny, such theses, and proposes instead a moral ideal of the unity of mankind.

A rejection of symbolism and similarly intentioned doctrines, together with a high estimation of the value of science,[5] might well be inoffensive, as Jarvie maintains - but only if certain pitfalls are avoided. In addition, it can combine not only with a moral vision but with a scientific theory of the underlying psychological unity of mankind.

Rationality and ethnocentrism

Initially Jarvie uses the term "rationality" in a broad way for all the characteristics of human beings that distinguish them from other animals (p. 22). He uses it, as I shall put it, for mankind's humanity. This in turn he identifies with the "capacity to apply reason to tasks" (p. 21). Described more fully, the capacity to apply reason to tasks is the capacity to "learn from experience, to store that learning and to recall and transmit it at will, and hence to learn about learning from experience" (p. 23).

Rationality so conceived is a form of psychological rationality; it concerns certain psychological capacities of individual humans. But Jarvie wants to "sociologize" the issue of rationality. Some motivation for doing so can be found in the suggestion that the social institution of scientific investigation facilitates the realization and development of, and embodies the results of, psychological rationality. In a derivative and social sense of the term, therefore, science can be said to be "rational". In addition, a concern with learning from experience as a psychological capacity already points beyond itself - towards methods and standards for such learning, and the like. People not only learn from experience, they also learn about, and how best to, learn from experience. "Rationality", Jarvie writes, "tied as it is to learning from experience, is in turn tied to assessing learning from experience" (p. 49). And he thinks that knowledge about and methods and standards for learning from experience are embodied in social institutions. Through their relationship to rationality or learning from experience in the primary psychological sense, these institutions can also be assessed for their rationality.

But Jarvie goes further. At times he seems to want to jettison the psychological sense of the word "rationality" altogether. Rationality, he writes, "is externalized in the social organization, not internalized in individual capacity" (p. 31). In the same vein, and in support of his view that rationality is "to be taken socially, as a characteristic of social systems" (p. 54), he argues against the psychological views that rationality is a characteristic of either mental processes or products (pp. 54-61).

I have tried to indicate the motivation for going beyond an exclusively psychological notion of rationality. But why go to another extreme and seek an *exclusively* social notion? The answer, it seems, is in order to have a substitute for symbolism or relativism as a barrier against insulting ethnocentric judgements. Clearly, if rationality is not an individual but a social characteristic, then "Individual men are not ranked by rational capacity, social formations are" (p. 30). Similarly, Jarvie suggests, if

> the rationality of science consists not so much of thought processes of a special and exclusive kind, but of a means of organizing thought, of a set of social institutions, then we need deny no society some rationality and potential for more. To say that this society invented the transistor but that this one did not is no slur; and it need be no slur to say that this society invented the social institutions of science and that this did not.[6]

So much must suffice here to trace, in broad outline, Jarvie's course,from his initial identification of mankind's humanity and psychological rationality, through sociologized rationality, to a destination supposed to be respectful rather than disrespectful of other people and peoples.

But in my view, each of these stages is problematic. To start with, rationality and mankind's humanity are quite different. Jarvie, moreover, provides only very weak arguments for identifying them. The arguments consist in part of criticisms of two alternative views, in part of a defense of his own view. I shall consider these in turn. The alternatives he rejects are the view that man is the tool-using animal, and the view that man is the language-using animal.[7] The first is fairly easily disposed of on the ground that it does not provide a sharp demarcation of humans from other animals; many other animals make and use tools.

The second is another matter altogether. In rejecting it, Jarvie first claims that focusing on language shifts the problem from the demarcation of man from

177

other animals to that of the demarcation of human from animal languages or proto-languages (p. 22). That may be true, but it is not a compelling objection. Perhaps the shift is entirely appropriate; perhaps human language is both distinctive and provides the best clue there is to human nature or to mankind's humanity.

But instead of considering this, Jarvie instead proceeds to condemn the shift as involving a reformulation of the problem as one concerning "the emergence of language" (p. 22). Yet such a reformulation need not be involved. Questions about the nature of language are separable from ones about its origin. And even if that were denied, the consequences would not be those Jarvie draws. A view of language might hold, as Chomsky's does, that human language differs in type from animal "languages". Such a view might even imply, as Jarvie suggests it does, that "the language-using animal has to emerge fully formed with his capacity to learn language in place" (p. 23). Nonetheless, the view need not be committed to what Jarvie rejects as an "anti-evolutionist" position (p. 23). Although the doctrine of gradual change has been central in orthodox evolutionary theory, theorists like Eldredge and Gould, who question it, are not anti-evolutionists.[8] Rather, they want to revise evolutionary theory so that it accommodates more precipitate change and thus better fits the fossil record. Language, therefore, might have evolved suddenly rather than gradually.

The idea that man is the language-using animal survives Jarvie's attack, then. Furthermore, there are well-known and good reasons for thinking that human language is quite unique, and so for thinking that human uniqueness is indeed either based on or closely tied up with the nature of human language. On the one hand, the evidence for gorillas or chimpanzees, let alone whales or dolphins and the rest, having anything like the human linguistic capacity, is at best ambiguous.[9] On the other hand, Chomsky has clarified a number of essential features of language, one at least of which leaves little room for doubt that human language is species-specific. In particular, typical language users are able to produce and understand novel sentences, sentences they have not encountered before. As there is no end to the novel sentences users can produce or understand, human languages are unlimitedly vast in extent. And since sentences are discrete objects, it follows that human language, unlike any known non-human communication system, is "in principle an infinite discrete system".[10]

At the very least, then, the possibility of a linguistic view of mankind's humanity remains in the field.[11] Nor are its attractions diminished by the arguments Jarvie supplies for his own view of the matter. These amount to little more than a few remarks about man's two-pronged evolutionary strategy. Man both adapts himself and modifies his niche (p. 23), he notes. He concedes that other animals might bring reason to the tasks of "survival and thrival" (p. 23), but claims that man's capacity "to learn to improve his application of reason to

178

tasks is unmatched" (p. 23). Is it not unmatched precisely because of the difference language makes?

In any case, even if man's ability to learn from experience or to learn to apply reasons to tasks is unmatched, it still does not follow that mankind's humanity can be identified with this ability. Rationality as that which distinguishes man can only be identified with "the capacity to learn from experience..." if this capacity includes, or at least underlies, everything that distinguishes man, not just part of it. Jarvie's specification is incomplete. What, for example, of man's apparent uniqueness as an animal that develops and engages in religious rites and rituals? It may be granted that this human trait springs, at least in part, from the same source as science. It can even be granted that it shares important or even essential features with more scientific activities, as intellectualists claim. Nonetheless, it would also seem to have its own peculiar properties; and these properties point to an exclusively human capacity or need. What too of the ability to create pictorial and plastic representations? It is presumably no accident that these first appear on the scene with *Homo sapiens sapiens*, and always and only accompany the presence of humans. And the ability to make music? And then there are attitudes and emotions like Humean sympathy, or ordinary infatuation, love, envy, vengefulness, and so on, through a very wide gamut. None of these can be attributed sensibly to other animals. If all the capacities, needs, activities, and so on just mentioned are somehow included in, or dependent upon, the capacities to learn from experience, to store, recall, and transmit learning, and to learn about learning from experience, Jarvie does not tell us how.

In short, mankind's humanity cannot be identified with human psychological rationality. Moreover, if the two are identified, then entirely unjustified ethnocentric judgements result. For then the claim that science is "the supreme embodiment of mankind's rationality" (p. 21) has two meanings. It means, first, that of all social institutions, science is the one that best facilitates the realization or development of the capacity to learn from experience, and embodies its best results. This would seem to be Jarvie's intended point. Nonetheless, the claim has a second meaning: science is the supreme embodiment of mankind's humanity. That is to say, science supremely embodies everything distinctive of humans; it is the least animal of all human achievements, indeed, the benchmark of *human* achievement. Scientific people, peoples, and cultures have nothing to learn from others, being globally superior to them.

It might be objected that the notion of rationality featuring in the claim that science is the supreme embodiment of mankind's rationality is the social rather than the psychological notion. Therefore, even if the psychological notion is ambiguous, the claim itself need not be ambiguous. But the ambiguity infects the social notion as well. Contrary to some of Jarvie's claims, the social and psychological notions remain closely linked; the former derives from, and only makes sense by reference to, the latter. If the links between the two were cut completely, then unless a new and purely social definition of the term "rationality" were supplied, we could have no idea at all what the "social-rationality" supremely embodied in science is supposed to be. It cannot be whatever it is that happens to be supremely embodied in science, for then the supreme embodiment claim would be trivial. As it is, no new definition is supplied, and the links are clearly retained. "Science is a paradigm of rationality", Jarvie himself emphasizes, precisely because "it is the result of special kinds of social institutions that foster if they do not impose the critical and open-minded attitude of wanting to *learn from experience*".[12] Both the psychological and the social notion have to do essentially with rationality as learning from experience. Through this in turn, and the ambiguity it involves, both of them have to do with mankind's humanity.

The claim that science supremely embodies the results of, and facilitates the development of, the capacity to learn from experience may be plausible. But the claim that science supremely embodies, or facilitates the development of, everything distinctively human, is highly implausible. The most that arguments like those of Horton or Jarvie prove is the superiority of science in some *specific* or *local* respect: as regards description, explanation and prediction, as in Horton's case, or as regards learning from experience, as in Jarvie's. An argument for the *local* superiority of science is not in itself one for the *global* superiority of science or of scientific cultures.[13]

Horton, for example, often claims there are other respects in which African traditional thought is superior to Western science. Local superiorities leave room for the mutual respect for each other's achievements by the members of different groups.

Now I think Jarvie does in fact advocate something very like this. Nonetheless, it is best to preclude explicitly the offensive ethnocentric interpretation of the claim that science is the supreme embodiment of mankind's rationality. And this is to be done not by sociologizing the notion of rationality, but rather by not conflating mankind's rationality and humanity. For it is the *local* and not the *social* nature of rationality that is crucial for anything like an acceptable interpretation.

This shows too that an exclusively social notion of rationality offers little if anything by way of a barrier against offensive ethnocentric judgements. For we can credit every society with some rationality and potential for more, as Jarvie puts it, in a sense of rationality intimately connected with the psychological one just as much as in any exclusively social sense. It remains the case that science may be more developed in some cultures or societies than in others, and that it is a distinctive achievement of Western culture. Certainly, if arguments against symbolism like Jarvie's or Horton's are sound, and if science is indeed superior to other knowledge systems in some local respect, then scientific cultures or societies may be said to be superior to others in that particular, local, respect. But such a judgement need not be merely ethnocentric in any illegitimate way. Other cultures or societies might be said - equally non-ethnocentrically - to have their own distinctive achievements, not ones more rational than science, but superior to Western achievements in other - and, arguably at least, more important - local ways.

Besides isolating the valuable aspects of Jarvie's conception of rationality from tainting associations, there are other advantages in not conflating mankind's rationality and humanity. Most importantly, room is left for more egalitarian conceptions than his of mankind's humanity. The conflation stacks the cards in favor of science, scientists, and scientific cultures. But if human uniqueness is grounded in, for example, the capacity for language, then science, whatever its other virtues, would not be the most *human* of all human achievements. Religion, art, folk wisdom, magic, science, and so on, would all seem to depend equally upon, or to exhibit or further, the capacity to use language. They would be on a par as far as mankind's humanity, so conceived, is concerned.

The psychological unity of mankind

Complementary to questions about the nature of human rationality there are questions about the rational or psychological unity of mankind. Is rationality always and everywhere the same, or does it come in different varieties? Are some people, or some peoples, more rational than others? Is the innate psychological endowment of certain groups of people not merely different from, but somehow superior or inferior to, that of others, as many racists claim?

Such questions, for Jarvie, are basic in anthropology. For he holds that"all anthropological activity", from Hebrew, Greek and Roman, through to contemporary, "consists fundamentally in meditations upon the problem of the unity of mankind" (p.9). At the same time, however, he chooses neither to assert nor to deny theses of the basic rational or psychological unity of mankind. In part his reason is a suspicion that "we cannot ascribe a rational essence to man such that it is uniformly distributed amongst all the members" (p. 25). But his main motivation is his belief that claims about underlying psychological unities or diversities among humans are metaphysical; they can be confirmed but not refuted by evidence (p. 14).

Concerned about the fundamental and sensitive problem of human unity, then, and believing a direct attack on it to be out of place, Jarvie mounts a rearguard action - lest it transpire that, for example, "human beings are of several families with somewhat different endowments" (p. 14). His preferred strategy consists in a shift away from descriptive unity claims towards a normative or moral version of the unity of mankind. This now becomes a "proposal for how to act and think" (p. 14), a proposal at one with the "noble ideal" that Jarvie supports of incorporating all peoples into our moral community and according them "the dignity and respect we accord our own members" (p. 85). In short, Jarvie mounts a defense at two removed from the front. He does not deny that, for example, human beings are of several families. And he does not take up the next position according to which if there are such families, then they exhibit no significant innate psychological differences.

But I think that a more aggressive strategy is possible. For I think that scientific rather than just metaphysical views of the psychological unity of mankind are possible. Or, at least, they are possible as long as scientific psychological theories are possible.

Despite learning from experience being central to Jarvie's conception of human rationality, he neglects one of the most striking of all human learning feats - the acquisition of language. Perhaps that is because he goes along with the widely held current view that language is predominantly not learned by or from experience at all, at least not in any usual empiricist interpretation of such learning.[14] Nor can Popperian empiricism, which is Jarvie's underlying view, explain language acquisition.[15] Rather, language is predominantly innate. But it is more likely that Jarvie's neglect of this topic results from his general attempt to side-step language and linguistic matters - as we saw in the last section. Yet language acquisition in particular, and psychological phenomena and issues more generally, are central to the very problem that he himself characterizes as fundamental in anthropology, the problem of the unity of mankind.

The basic point here is by no means tied to, but it is probably best made by reference to, language acquisition. It may be granted that there is much room for debate about the precise nature of the innate equipment that must be attributed to the mind in order to explain such facts as that children learn languages of unlimitedly vast scope on the basis of meager data and minimal instruction. But that is irrelevant. The crux of the matter is that psychological theories about the detailed nature of such equipment are theoretical or explanatory theories of the usual scientific sort. The innate mechanisms, structures, and so on that such theories posit are of the nature of theoretical entities. The explanatory success of such theories is evidence for the existence of the entities and structures they posit. And such theories are falsified or refuted in the usual way: by phenomena with which they are incompatible or which they cannot explain.

It follows that scientific theses of the psychological unity of mankind are possible. Suppose that L, say, is a refutable and explanatory - thus scientific - theory of language acquisition. Suppose too that, in order to explain how children acquire language, L attributes to them a set of innate mental or psychological structures, mechanisms, and processes. Of course, if L is to be either explanatory or refutable, it will have to be precise and specific about this innate equipment. For the sake of the present argument, we may assume also that this condition is satisfied. Then the following is a refutable thesis about the psychological unity of mankind:

L": All (normal) human minds are as described by L.

If a human language were discovered the acquisition of which could not be explained by L, then L" would be refuted.

The same kind of reasoning also shows that refutable theses of the psychological diversity of mankind are possible. Suppose that L1 and L2 are incompatible theories of language acquisition. Then the claim that the members of some specific group of humans, say A, have minds that are described by L1, and the members of another specific group, say B, have minds described by L2, is a refutable thesis of the psychological diversity of mankind.

Though I have concentrated on theories of language acquisition, what I have argued about them holds in principle for scientific psychological theories in general. Such theories provide bases for refutable psychological unity theses. Scientific theories of perception, for example, or of aspects of cognition and cognitive processing, can feature in scientific theses of the psychological unity of mankind just as L features in L'. Jarvie's claim that views about the underlying psychological unity or diversity of mankind are metaphysical in the sense of not being refutable must be rejected.

If any scientific knowledge of the human mind, including basic human psychological rationality, is possible, then scientific knowledge of the unity of mankind is possible. And as more scientific knowledge of the human mind develops, so more theses of the psychological unity of mankind become available. But the primary role of scientific psychological theories is of course to cast light on human psychological nature; they feature only secondarily in fundamental anthropological theses of the unity of mankind. In their primary role, moreover, they may be expected to illuminate the underlying structures and the characteristic mechanisms and processes involved in learning from experience, and in storing, recalling, and transmitting such learning. In so doing they would provide us with scientific knowledge of basic human psychological rationality.

Conclusion

Limiting the purview of psychological rationality while increasing the attention given to language, and through that, to more general psychological phenomena, strengthens Jarvie's view. On the one hand, it removes from the view a taint of insulting ethnocentrism, and at the same time allows for the incorporation in it of an egalitarian and also scientific view of human uniqueness. On the other hand, it makes for the incorporation of his view itself in a wider - and, I think, more reliable - perspective, one that offers not merely a metaphysical and moral but also a scientific conception of the underlying psychological unity of mankind. Such a conception would be an important step towards the ultimate defense of Jarvie's position against accusations to the effect that it is an expression of, or expresses, parochial bias. For it would be an important step towards showing that science itself results from universally the same basic human psychological constitution operating in different circumstances.

Notes

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1. In *Africa*, XXXVII, Nos. 1 and 2, 1967, 50-71 and 155-87. Shorter version in B. R. Wilson (ed.), *Rationality*, Oxford, Basil Blackwell, 1970, 131-171.
2. I. C. Jarvie and Joseph Agassi, "The Problem of the Rationality of Magic", *The British Journal of Sociology*, 18, 1967, 55-74.
3. I. C. Jarvie, *Rationality and Relativism*, London, Boston, Melbourne and Henley, Routledge and Kegan Paul, 1984.
4. Dan Sperber, "Apparently Irrational Beliefs", in M. Hollis and S. Lukes (Eds.), *Rationality and Relativism*, Oxford, Basil Blackwell, 1982, 149-180, pp. 178-80.
5. I neither defend nor attack either of these. My concern is more with their implications. And although Jarvie's attack on relativism is central, I do not assess it here.
6. Jarvie, *op. cit.*, pp. 95-96. A similar motivation can be found in Jarvie and Agassi, *op. cit.*, pp. 69-70.
7. A third view, that man is the culture-creating animal, Jarvie regards as derivative from the first two.
8. For a brief review see R. Lewin, "Evolutionary Theory Under Fire", *Science*, Vol. 210, 21 November 1980, 883-887. See too S. Gould, "Darwinism and the Expansion of Evolutionary Theory", *Science,* Vol. 216, 23 April 1982, 380-387.
9. See, for example, T. A. Sebeok and Donna Jean Umiker-Sebeok, *Speaking of Apes: A Critical Anthology of Two-Way Communication with Man*, New York, Plenum, 1980.
10. N. Chomsky, "Knowledge of Language", in K. Gunderson (ed.), *Language, Mind, and Knowledge*, Minnesota Studies in the Philosophy of Science VII,

Minneapolis, Minnesota University Press, 1975, 299-320, p. 302. Other features that Chomsky emphasizes, like the freedom he claims is involved in the production of human utterances, as well as the relation of appropriateness that he says obtains between utterances and the situations in which they are produced, are more problematic than the feature of novelty. I therefore avoid recourse to them here.

11. Incidentally, an inchoate view of just this type seems to be involved in Jarvie's talk of the *storing, recalling,* and *transmitting* of learning. This part of his conception of man's distinguishing features he neglects, however. In considering the rationality of science, he focuses on learning from experience, not on storing, recalling, and transmitting learning. The same goes for the arguments he presents to support his view of mankind's humanity, as is shown below.

12. Or so I read the passage on page 63 from which I have quoted. (I take it that the word "not", which I have omitted, is a misprint and should read "but".) The italics are mine.

13. For a similar distinction between local and global superiority see Charles Taylor's, "Rationality", in Hollis and Lukes, *op. cit*, 87-105.

14. N. Chomsky, "Review of B. Skinner, *Verbal Behaviour*", *Language*, 35, 1959; *Aspects of the Theory of Syntax*, The M. I. T. Press, Cambridge, Mass., 1965.

15. S. Stich, "Between Chomskian Rationalism and Popperian Empiricism", *British Journal for the Philosophy of Science,* 30, 1979, 329-347; "Empiricism, Innateness, and Linguistic Universals", Philosophical Studies, 33: 1978, 273-286.

11

To follow a rule or to rule what should follow? Rationality and judgement in the human sciences

A. A. van Niekerk

The purpose of this paper is to explore the relevance and significance of the concept of judgement for a model of rationality in the human sciences. This will be done in discussion with Harold Brown (1988) who develops a model of rationality in which the concept of judgement is the key concept. I will argue that Brown makes a significant contribution to the search for a model of rationality which is better accomodated to the insights of postempiricist philosophy of science, but that, if we wish to apply such a model in the human (in which is incorporated the social) sciences, certain important modifications are required. For my understanding of the concept of judgement, I will also rely on the important insights of Hannah Arendt, to whom Brown (surprisingly) never refers.

1. The classical model of rationality

One of the main features of almost all traditional models of rationality is their reluctance to accomodate the concept and practice of judgement. Brown (1988: 3-37) provides us with an example of a traditional model which clearly illustrates this reluctance. Let us call this traditional model the "classical model". This model, which enjoyed great popularity in the history of science and philosophy, requires that a rational decision, action or belief must be based on an evaluation of relevant evidence by the application of appropriate rules (Brown, 1988: vii). It specifies three conditions that have to be met by beliefs which claim rational status:

1) universal validity,
2) necessary relations between components of arguments, and,
3) compliance to rules:

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The first condition entails that, given sufficient information on which to base a solution to a problem, all rational agents have to reach the same solution. But the possibility exists that these agents might reach the same conclusion as a result of a massive coincidence or as a result of ignorance or indoctrination, rather than through reasoning. A rationally acceptable condition must therefore, secondly, follow with *necessity* from the information given (cf. Kant, 1972: 43-4). The third condition is that the rationality of any conclusion is determined by whether it conforms to appropriate *rules*. According to the classical model, "when we prove from a starting point to a conclusion in accordance with a set of rules, we free ourselves from the arbitrariness that is characteristic of nonrational decisions" (Brown, 1988: 17). "Rules" is the key concept in this model, because if we have universally applicable rules, then all who begin from the same information must arrive at the same conclusion, and it is these rules that provide the necessary connection between our starting point and our conclusion.

2. The tension between judgement and the classical model

An analysis of the concept of judgement makes clear why this "faculty of thinking" has traditionally been regarded as irreconcilable with the procedures and fruits of cognitive reason, when the latter is understood in terms of the above or closely related models.[1]

Jugdement is defined by Brown as "the ability to evaluate a situation, assess evidence and come to a reasonable decision *without following rules*" (1988: 137) The first and foremost characteristic of judgement, which immediately places it at loggerheads with the first two requirements of the classical model, is its focus on the *particular* and the *contingent,* rather than on the *general* (universal) and the *necessary.* Hannah Arendt's analyses of this feature of judgement are perhaps more elucidating than that of Brown (cf. Arendt 1961, 1973, 1978, 1982).

Time and space do not allow me to dwell on the particularities of the context in which Arendt develops her concept of judgement.[2] Suffice it to say that she discovered in the concept of judgement, through a highly original but also controversial interpretation of Kant (cf. Arendt 1982: 7-78), the key to the understanding of political life. Her point of departure is her analysis of the difference between *truth* and *opinion*, and her insistence that, while the search for truth, which is the leitmotiv of human cognition, has no place in politics, opinions are the very stuff of politics.[3] Opinions have to be *formed* by individuals and must be tested and enlarged where there is a genuine encounter with different opinions. Only the force of the *public* argument can serve as a test of the adequacy of all opinion.[4] The formation of opinions requires a community of equals (and their equality is not some "inborn right" or "capacity",

157

but the public creation of the polis), the imagination to represent other viewpoints and the courage to submit opinions to public exposure and test.

Politics - the public life of the polis - is therefore fundamentally defined by *debate,* i.e. the public exchange and testing of opinions. It is a "sharing-the-world-with others" in which the capacity to *judge* plays a pivotal role. In fact, writes Arendt, "Judging is one, if not the most important activity in which this sharing-the-world-with-others comes to pass" (1961: 221). It was more than anyone else Kant who provided us with insight about the distinctiveness of judgement - and he provided it, significanctly, when he was examining the phenomenon of *taste*[5] (Kant, 1972: 37-81). What Arendt has in mind, is Kant's analysis of "reflective judgement", that mode of thinking particulars which does not subsume particulars under general rules, but ascends from the particular to the universal, instead of the other way round. It involves discriminating and discerning the particular in its particularity. Rather than sight or hearing, Kant saw that it is above all *taste* which provides the vehicle, and therefore the model, for judgement: "... only taste and smell are discriminating in their very nature and only these senses relate to the particular qua particular: all objects given to the objective senses (i.e. sight and hearing - AAvN) share their properties with other objects; they are not unique. Moreover, the it-pleases or displeases me is overwhelmingly present in taste and smell. It is immediate, nonmediated by any thought or reflection" (Arendt, 1978: 264).

The facts that taste provides the model for the understanding of judgement and that judgement is identified as the capacity to discriminate the particular in its particularity, give rise to two other insights of Arendt on this matter which is of fundamental importance for our purposes. The *first* is that judgement, in spite of its focus on the particular and its subjective enactment, is irretrievably *intersubjective.*

The significance of Kant's basing of judgement on taste, is that taste, as the model of judging, is not to be identified with "private feelings"; it is the very opposite.[6] Taste is a kind of "sensus communis"; a "community sense" - the sense which fits us into human community. Arendt quotes Kant when he says that matters of taste (or "the beautiful") interest us *only in society*. "... A man abandoned by himself on a desert island would adorn neither his hut nor his person... (Man) is not contented with an object if he cannot feel satisfaction in it in common with others...In matters of taste we must renounce ourselves in favour of others...the non-subjective element in the non-objective senses[7] is intersubjectivity. (You must be alone in order to think; you need company to enjoy a meal). Judgement, and especially judgements of taste, always reflect upon others and..take their possible judgements into account" (Arendt, 1978: 265-266).

Judgement is therefore an "enlarged mentality" ("eine erweiterte Denkungsart") where one is able to "think in the place of everybody else"

(Arendt, 1961: 220). The judging person, as Kant says, "can only 'woo the consent of everyone else' in hope of coming to an agreement with him eventually (1961: 222). Judgement is a mode of thinking that is not to be identified with the expression of private feelings, which is capable of dealing with the particular in its particularity, but which nevertheless makes the claim to communal rationality.[8]

The *second* point is that judgement, because of its focus on the particular, is always to be *clearly distinguished from cognition* and particularly from the type of universality characteristic of cognitive reason. We have seen that Arendt fears the operation of cognitive reason in public life. Cognitive reason strives for truth; politics deal in opinions. To impose the procedures, tests of validity, and the demands that are appropriate to cognitive reason's search for truth on politics is in effect to destroy politics with its essential and non-reducible plurality and variability of opinions. As Bernstein succinctly states: "The thrust of her (Arendt's) political thinking was to provide an apologia for the political life against the claims of the philosophers" (1986: 227). The introduction of factual truths to the life of the polis generally ends up with a blurring of the distinction between facts and opinions, and modern man's experience of totalitarian regimes testifies to the disastrous effects of that most undesirable propensity.

3. The untenability of the classical model

Arendt's analyses, though providing us with illuminating insights into the concept of judgement, make clear why the practice of judgement is irreconcilable with the classical model of rationality. The presumed tension between rationality and judgement is the result of judgement's focus on *particularity* and *contingency* vis a vis the classical model's appreciation of *universality, necessity and rules.*

We now turn to the question as to whether the classical model provides us with a conception of rationality which can be applied with any measure of coherence or consistency. The answer is, of course, negative. Two sets of considerations motivate such a conclusion, and both point to the necessity to recognise the role and function of judgement in all cognitive processes and circumstances which are liable to evaluation concerning their rational status.

The *first* reason why the classical model is at best an incomplete and at worst an unsuccessful account of the nature of rationality, concerns the epistemological presuppositions and logical requirements of the model. The problem in this regard is that the model presupposes a *foundationalist epistemology*. The provision of reasons for beliefs, on this model, consists in "grounding" the proposition believed by demonstrating the link between this proposition and some "point of departure", and by providing the rules that establish the connection between the information we begin with and the proposition believed. Rationality, in other words, consists in providing foundations for beliefs. Two questions immediately arise about this approach:

(1) on what basis do we select the information from which to begin (or: what is the foundation for the proposed foundation?), and
(2) on what basis do we select our rules?

The classical model requires that we begin from an appropriate starting point and use appropriate rules. The question is: How is the appropriateness of this starting point and these rules to be decided, and by whom?

This question remains an insuperable obstacle for a tenable and coherent defence of foundationalism and, therefore, to the classical model as based on foundationalism. Hans Albert shows that the requirement of providing foundations for foundations inevitably gives rise to the so called "Münchhausen trilemma" of accepting either an infinite regress in the grounding process, or a logical circle of providing statements, which have already been identified as in need of justification, or the *arbitrary* breaking off of the justification process at some "Archimedean point" of departure, i.e. a proposition which is reputedly self justifying because it is "self-evident" (Albert, 1985: 18). The last procedure, which has consistently been the most popular in the history of the classical model, is clearly inconsistent with the requirements which the model itself sets for rational status. It boils down to a "recourse to dogma" (Albert, 1985: 19) since it implies the elevation of *one point in the grounding process to the last point*. Thereby the principle of sufficient justification - the main methodological principle of the classical model - is arbitrarily suspended.

And even if we could reach consensus about a set of foundational propositions, the same problem would also arise if we are required to provide grounds for the rules that we use in the justification process. Brown, in his discussion of this matter, suggests that elementary logic apparently provides examples of rules that have, to adherents of the classical model, seemed beyond the pale of reasonable doubt. In his exploration of rules, which I cannot discuss in any detail here (cf. Brown, 1988: 72-78), he comes to the conclusion that it is not self-evident which rules of logic we should accept. Logic is dubitable; the rules of logic are subject to reconsideration and require justification. The fact that it is not self-evident which are the correct rules of logic, suggests that as long as we are working in terms of the classical model of rationality, we cannot accept the validity of a rule of logic without justification. The problem is that it is not at all clear where and how that justification is to be found.

The second consideration which motivates the untenability of the classical model is a close scrutiny of the *actual practice of science in history*. This aspect receives considerable attention in Brown's work (1988: 38-134). Examples which he discusses include the problem of "overturned refutations" in view of critical rationalism's insistence on falsifiability as the basic methodological rule of science.[9] The main illustrations of this point are, however, drawn from Kuhn and Lakatos' contributions about the "reasons" for scientists' changed commitments to different paradigms or research programs.

The choice for accepting a different paradigm in science is clearly a decision on the part of people working in the field. The choice is never conclusively "proven" to be correct, and there does not exist a sufficient body of rules for deciding whether a new approach should be accepted as a paradigm, or when this decision should be made. From the point of view of the classical model, such decisions are not rational.[10] It now becomes clear why someone like Hannah Arendt insists on separating judgement and cogniton (and therefore rationality). She never really took serious heed of what we have learned from post-empiricist philosophy of science, viz. that decisions or judgements play an essential role in science itself. This is what is important in Brown's appreciation of the concept of judgement. Because he recognises, on the basis of the findings of post-empiricist philosophy of science, the undeniable place of judgement in (specifically scientific) cognition, a new insight is established in the interconnectedness, rather than dichotomous seperation, of rationality and judgement.

The central point that Brown makes in that regard (Brown 1988: 95-113) can be summarized as follows: The rules in accordance with which scientific decisions are made change as science develops. This shows that these rules do not meet the conditions of universality and necessity imposed by the classical model, and thus that science does not meet the conditions for a rational activity embodied in that model. That leaves us with several options. We can conclude that science is not rational - as a small number of philosophers, led by Feyerabend (cf. his 1975 and 1978), have indeed done. Or we can continue to search for the rules that will allow us to maintain both the classical model and the rationality of science. (We have, after all, not proven beyond a shadow of reasonable doubt that no such rules will ever be found). This search has also been attempted by philosophers of science. Or we can conclude that the classical model of rationality must be complemented. I agree, with qualification,[11] with Brown that this is the option we must choose. The breakdown of the classical model of rationality does not imply, as some might fear, the breakdown of rationality itself. We have seen that, in terms of the logic of the grounding process which the classical model identifies with rationality, a point is reached where a decision has to be made for which no rules are available. This decision is a judgement. The main deficiency of the classical model is exactly its inability, in terms of its presupposed epistemology, to recognise the rational status of judgements.

4. Judgement in a model of rationality

We now turn to the positive characteristics of a model of rationality in which judgement, rather than rules, is the key concept. Many aspects of this model have been suggested by our analysis of the concept of judgement. Those suggestions will not at this stage be repeated or developed. I will now briefly summarize the main positive features of the judgement model that Harold Brown suggests, in order to proceed, in the last paragraph, to an evaluation of its applicability in the human sciences.

(a) A judgement model of rationality takes the notion of a *rational agent* as fundamental, and such notions as *"rational belief"* as derivative in the sense that a rational belief will be one that is arrived at by a rational agent (Brown, 1988: 185). This, according to Brown, is the direct opposite of what is the case with the classical model. The judgement model agrees with the classical model that beliefs ought to be based on relevant evidence and that we must be prepared to change our beliefs when the evidence changes. The difference is that in the classical model the central emphasis is placed on the logical relations between the evidence and the belief, while the role of the agent is minimized or left out. Rationality thus becomes a characteristic of "objective" beliefs or theories, rather than of agents who have to assess evidence. Brown's model takes the agent to be basic, and the way in which an agent deals with evidence in arriving at a belief to be determinative of the rationality of that belief for her. This, of course, involves considerable relativization of rational beliefs to individuals (as is also implied in other aspects of his model), though that, according to Brown, is not the same as a relativization of the notion of rationality.

(b) The characteristic feature of a rational agent is her ability to make judgements in those situations in which we lack sufficient rules to determine our decision. Thus the notion of judgement becomes central to our model of rationality. This entails that our ability to act as rational agents is limited by our expertise. This does not mean that only experts can be rational, but it does mean that in cases in which I lack expertise, there may be only one rational decision open to me: to seek expert advice.

The difference from the classical model is stark when we realize that it is not the availability of rules which enables rational action. Rather: we depend on our ability to be rational especially when we lack rules (Brown, 1988: 186). When rules are available, an informed agent will recognise that this is the case, and will apply those rules; it is when rules are *not* available, that we require rational assessment.

(c) Rational decision-making is a *socially mediated* rather than a rule-guided process. For a belief based on judgement to be a rational one, it must be submitted to the community of those who share the relevant expertise for evaluation against their own judgements (Brown, 1988: 187). This requirement complies with our deep-seated, pre-analytic conviction that it would be foolish for an individual to place total confidence in her own judgement. The model accepts that human judgement is under all circumstances *fallible* and that our best chance of eliminating error is by exposing our judgement to the critical scrutiny of other people, particularly those acclaimed for the prudence and sobriety of their knowledge in the field.

The requirement also emphasizes the undeniable social aspect of judgement, its reliance on intersubjective confirmation, as has been suggested by the analyses of Kant and Arendt. Says Brown: "In these terms, Robinson Crusoe alone on his island could exercise judgement, but he would not be able to achieve rationality. This is not because of some failing in his faculties, but rather for a reason akin to the reason why he could not play baseball, even though he could throw balls in the air, hit them with a bat, and run bases. On the model I am proposing, rationality requires other people - and not just any people, but other people who have the skills needed to excercise judgement in the case at hand" (Brown, 1988: 187.)

5. Rationality and judgement in the human sciences

Although Brown sometimes refers to problems which are specific to the human sciences (1988: 113-134), the model of rationality he proposes is overwhelmingly informed by his perceptions of the character of and developments within the natural sciences. That is not to say that Brown regards (natural) science as the exclusive paradigm for a model of rationality;[12] there are other human and cognitive endeavors which can lay a claim to rational status. With that I agree. Nevertheless, Brown is convinced that natural science provides the most important test case for a model of rationality because it provides the best examples of cases in which the most varied and reliable evidence is systematically gathered and deployed (cf. Brown, 1988: viii, 191, 207). In spite of his interspersed references to issues in the social sciences, he never addresses those aspects of the human sciences which complicate the formulation of a comprehensive model of rationality for both natural and human sciences seriously. I turn therefore, in conclusion, to the question of the applicability of Brown's model in the human sciences.

But before we do that, let us first make a few cursory remarks about the distinctiveness of the object and procedures of investigation in the human sciences. I regard the human sciences as an intrinsically *hermeneutical* enterprise. The human sciences explore the realm of history as opposed to that of nature. Its method of enquiry is *interpretation*, which is the attempt to grasp *meanings*. Meanings are the objectivations of the human *mind*, manifestations of human life in its continuous, historical creation of the products of human culture. Interpretation has a *triadic* structure; it is a process in which

1. a creative spirit or mind communicates some
2. "meaningful form" (Betti, 1962: 54) to
3. an interpreter,

and any account of interpretation has to accomodate all three components of this triade. The "meaningful form" or message which is the object of interpretation, is something which generates the suspicion of meaning, without it being prima facie clear what the meaning is. "This object", says Charles Taylor, "must, therefore, be a text, or a text analogue,[13] which in some way is confused, incomplete, cloudy, seemingly contradictory - in one way or another, unclear. The interpretation aims to bring to light an underlying coherence or sense" (Taylor, 1985: 15).

If we then ask about the relevance and applicability of Brown's judgement model of rationality to the human sciences, the first aspect that springs to mind is the fact that judgement, as we saw in Arendt's analyses, is a form of thinking which focuses on the individual, rather than the general. "Judgement is the faculty of thinking the particular" (Arendt, 1978: 271). This aspect of the judgement model is to be applauded when we take cognisance of the *historical* character of the human sciences in which interpretation represents the effort to discern the particular significance and unique sense of a constituted meaning (e.g. a text, a historical event, a work of art, a social structure). Steven McGuire writes in this connection: " 'Historicity' is a shorthand for the long-recognized problem (in sociology, i.e. the human sciences - AAvN) of authentically extracting generalities from unique and transient events. Each human interaction is a unique moment in history, so that generalizations across a number of interactions distort them or fail to preserve the uniqueness of each event" (McGuire, 1981: 179). According to Brown, judgement becomes necessary exactly when no rules are available. These situations appear much more frequently in the human than in the natural sciences, and they certainly do not only appear, as Brown sometimes seems to suggest (1988: 189-194, 207-224), during paradigm crises, as exemplified by the great debates between Galileo and Bellarmine or between Einstein and Bohr. The quest for universal validity guided by rules plays a very limited role, if indeed any role, in activities such as the interpretation of a poem or an historical document. The point is that, whereas judgement is a *continual* requirement in the rational behaviour of natural scientists, it is a *continuous* requirement - to the point of becoming an identifying characteristic - of human scientists.

Let us now turn to two other characteristics of the judgement model as summarized in the previous paragraph, viz. the priority of rational agents over rational beliefs, and the crucial role of experts in assessing the rational status of views and decisions. On these two scores, the peculiarities of the human sciences call for important modifications to Brown's model.

As regards the first aspect, the priority which Brown affords to rational agents over rational beliefs puts his model much more in line with the requirements of the human sciences than the classical model's reverse priority.

The trouble with Brown's model is, by his own admission (Brown, 1988: 185), its relativization of the concept of rationality to the beliefs, behaviour and judgement of *individuals*. Brown is quite explicit on this: "... on the model I am proposing the predicate 'rational' characterizes an individual's decisions and beliefs, it does not characterize propositions and it does not characterize communities. A community of individuals with the appropriate expertise is, on this model, necessary for an individual to arrive at a rational belief, but it is the individual's belief that is rational, not the community" (1988: 193).

The final criterion by which the rational status of the individual's judgements is determined, is the agreement of experts. We will not pursue the obvious difficulty which this view calls to mind, viz. the historical fact that in a certain epoch or "paradigm" the best acclaimed "experts" have so often been spectacularly wrong. The problem I want to pursue, is one which represents one of the most pressing concerns of the social sciences, namely social relativism.

The problem is generated by two concerns, neither of which is adequately accounted for by Brown's model. Firstly, even if we admit that judgement, as validated by the judgements of experts, is the pivotal characteristic of rationality, it cannot be denied that the content and range of an individual's judgements are significantly constrained by the conceptual equipment that she inherits from the historical community to which she belongs. I doubt whether Brown would deny this; his appropriation of Kuhn's idea of "paradigms" in the practice of science and the status which historically positioned experts receive in his book, suggest that he should agree. To adopt Gadamer's terminology in this respect, one might say that judgements are in no way independent of the authority of a *tradition*; the possibilities of judgement are conditioned by the prejudices which we inherit from our historical situatedness.

The second concern is that human, and particularly *social* scientists interest themselves in the possibility of trans-cultural communication. It is all very well to rely on the validating judgement of experts in the disputes between adherents of different "paradigms" within the *one tradition of scientific enquiry*. What, however, happens when *judgements* have to be made about the "rationality" of conflicting behavioral, epistemological or ethical practices of members and communities of *different cultures*? Who, in this case, could possibly act as "experts" against whose judgements we can check our own, other than people who belong to either of the compared cultures and whose judgements are therefore significantly conditioned by the prejudices of their own historical environment? Brown's model suggests that judgement should replace rules. But it is very doubtful whether the old anthropological question of whether there are cross-culturally valid standards (in the sense of rules) of rationality can be *better* approached by asking for cross-culturally valid expertise!

Brown's line of argument, when he eventually comes around to addressing these issues on pp.195-97, is palpably inadequate.[14] He denies that he defends a position which maintains that every social group is automatically rational. "On that view", says Brown, "rational behaviour is rule following behaviour and any group following socially accepted rules is ipso facto rational according to their own standards. Our new model involves tighter constraints on rationality than simply following rules, and while many different sets of beliefs may all be rational on this model, the question of whether any particular beliefs are in fact rational must be decided *by examining the way in which those beliefs were arrived at* (1988: 175). But this is merely begging the question. Because what is at issue in anthropology's question about trans-cultural standards of rationality is exactly "the way in which beliefs are arrived at". One would, for instance, expect that Brown requires validation by experts as the crucial test for being satisfied with the "examination of the way in which beliefs were arrived at". But the crucial question for the social scientist is why "validation by experts" is to be preferred to other methods of confirmation of beliefs in other cultures. Brown's work, because of its reliance on natural science as the paradigm for rationality (cf. 1988: 191), reveals an ethnocentric bias towards western-scientific-theoretical procedures of knowledge acquisition and validation, and does not really address the problem of social relativism seriously enough.

To examine what is really at issue in the debate about social relativism, Charles Taylor's discussion of Winch's celebrated *Understanding a primitive society* (Winch, 1964: 307-24) is particularly illuminating. According to Taylor, the crucial difference between our society and that of the Azande tribe who, according to Winch, is engaged in a different language game ("Zande notions of witchcraft do not constitute a theoretical system in terms of which Azande try to gain a quasi-scientific understanding of the world" - Winch, 1964: 315), is that we as Westerners have this activity of *theoretical understanding* which seems to have no counterpart among them (Taylor, 1985: 136). Theoretical understanding aims at a "disengaged perspective", as is suggested by the Greek word "theoria" ("contemplation"). This disengaged perspective implies that we, unlike the Zande, "are not trying to understand things merely as they impinge on us, or are relevant to the purposes we are pursuing, but rather grasp them as they are, outside the immediate perspective of our goals and desires and activities" (Taylor, 1985: 136). This kind of activity implies two connected things: that we come to distinguish this disengaged perspective from our ordinary stances of engagement, and that one values it as offering a higher - or in some sense superior - view of reality. The ability to adopt a theoretical approach is, in an important sense, the cause of the fact that the question about social relativism originally appears in Western culture, and not in others. What exactly enables our culture to adopt this approach is a matter of considerable dispute. An author like Jack Goody suggests that the difference between a "primitive"society and ours should be understood in terms of the opposition between "oral" and "literate" societies (rather than in terms of more familiar opposites like "traditional" and "modern" or "closed" and "open" societies) (Goody, 1977). A *literate* society will be able to develop critical attitudes more reflectively and self-consciously, because the preconditions in terms of which members of such a society understands the world, are written down and can be re-examined for error. Texts are available for careful scrutiny, so that reading a text is more impersonal and, by implication, more "objective" than listening to a speaker, as in oral societies (Macdonald and Pettit, 1981: 53). Speech is tied to the involvement in specific situations in ways in which a text is not. Says Goody: "A concern with rules of argument or the grounds for knowledge seems to arise, though less directly, out of the formalisation of communication (and hence of "statement" and "belief") which is intrinsic to writing. Philosophic discourse is a formalisation of just the kind one would expect with literacy. "Traditional" societies are marked not so much by the absence of reflective thinking as by the absence of the proper tools for constructive rumination" (Goody, 1977: 44).[15]

The point is, we do not find things like writing and the theoretical, contemplative, disengaged approach to understanding and problem solving in every culture, and this makes for an immense difference in the things we think and say.

These are considerations which are clearly not taken into account by Brown's naive proposal that the rational status of beliefs can be decided by "examining the way in which those beliefs were arrived at". In different cultures we arrive at beliefs in (sometimes) completely different ways. Much more has to be established about how judgements concerning the rationality of these different ways can be made, and particularly about how the "expertise" which is supposed to evaluate these judgements can be identified in a way that is acceptable to members of different cultures who are being compared (if, indeed, these cultures are at all interested in making such comparisons).

It is not my intention in this paper to develop a position which can satisfactorily clear up this deficiency in Brown's model. It suffices to say that I do believe that a judgement model has better potential to account for the rational status of trans-cultural interpretations and evaluations than the classical model. And in our judgements, we are not unavoidably committed to ethnocentricity. Neither do we have to accept Winch's incorrigibility thesis. I am attracted to Charles Taylor's plea for the possibility of a mutual exposure of different cultures to one another - an exposure in which the languages through which these confronted cultures traditionally understood themselves, are continuously challenged. That creates the possibility of developing a language for mutual

137

understanding which is neither their language nor ours, but " a language of perspicuous contrast" (Taylor, 1985: 125). "This", says Taylor, "would be a language in which we could formulate both their way of life and ours as alternative possibilities in relation to some human constants at work in both" (1985: 125). One of these "human constants", I would like to argue, is the tendency and desire to *judge* and to test that judgement, not necessarily against the "knowledge" of "experts", but against the *wisdom as well as the uncertainty* of the collective experience of a culture which, also because of its exposure to ours, is in a state continuous historical flux, just as ours is in flux because of its exposure to theirs. It would be language, Taylor continues, "in which the possible human variations would be so formulated that both our form of life and theirs could be perspicuously described as alternative such variations" (1985: 125). I would add: the desirability and meaningfulness of these variations for particular problems and circumstances will consistently have to be judged anew and evaluated in terms of the historical and contemporary experience of both. Such a language of contrast might show the other culture's language of understanding to be distorted or inadequate in some respects, or it might show ours to be so (in which case we might find that understanding them leads to an alteration of our self-understanding, and hence our form of life - a far from unknown process in history), or it might show both to be so (Taylor, 1985: 126).

In the absence of rules - and nowhere are rules so conspicuously absent than in the human sciences - we have to rely on good and informed judgements. The effort to keep these judgements rational depends on our faith in a common humanity which, although characterized by severe cultural diversity, holds the promise of mutual enrichment through the scrupulous development of a language of perspicuous contrast. To this language we can and must contribute, but from it we can and must receive and learn, since it embodies the vehicle of a mutual understanding which will be, for us and for others, a new self-understanding.

Notes

1. It certainly is not my intention to suggest that the classical model, as explained in this paper, is the only traditional model of rationality. It merely represents an important tradition in Western philosophy. Its presentation in this paper is clearly an oversimplified schematization, along lines suggested by Brown, for the sake of argument and a very generalized clarification. A related, though in many respects different, explication of the same model can be found in Albert, 1985: 3-38.

2. For this purpose, the reader should consult the excellent discussions of Richard Bernstein in his 1983: 207-216 and 1986: 221-226.

3. "There have always been those who have sought to impose the standards appropriate to truth and the peculiar element of coercion that truth carries to the realm of politics - typically with disastrous results. For to impose the procedures, tests of validity, and the demands that are appropriate to truth on politics is in effect to destroy politics with its essential and non-reducible plurality and variability of opinions" (Bernstein, 1986: 227).

4. It is significant to note that Arendt sees *debate* as the "very essence of political life" (Arendt, 1961: 241), instead of, for instance, the exercise of *power*. Politics operates through persuasion, and the latter can only be accomplished within the confines of a defined *public space*, the "polis", which guarantees not only the the possibility of debate, but also the exercise of human freedom as such.

5. Arendt's interpretation of Kant is, indeed, controversial, and its adequacy cannot be discussed here. She makes the striking claim that it is in the first part of Kant's *Critique of Judgement*, where he deals with aesthetic judgements which are normally believed to be the furthest removed from politics, that we find his "unwritten" political philosophy (Cf. Arendt, 1978: 255-72 and 1961: 220 ff.)

6. Arendt acknowledges that there is a tension between the emphasis on taste as the model for judgement, and the fact of its intersubjectiveness. "...the disturbing thing about matters of taste is that they are not communicable" (Arendt, 1978: 265). She even calls this tension a "riddle".

7. E.g. taste and smell, as opposed to the "objective sense" such as sight, hearing and touching.

8. Cf. Arendt, 1961: 221

9. Brown's point is that the history of science proves that falsification is not nearly as simple a process as Popper conceives it to be, and does therefore not provide any clear grounds for believing that science is more likely to progress if we follow a policy of rapid elimination of hypotheses in the face of contrary evidence than if we follow a policy of "tenacious protection of apparently refuted theories, while we search for supplementary hypotheses that will allow us to protect those theories" (Brown, 1988: 93). Nor is there any reason to believe that we can put clear limits on how long it is legitimate to seek a means of protecting an hypothesis. In the end, scientists have to rely on decisions in these matters. What is disputed here, is the claim that if these decisions are rational, they are governed by some unstated rule.

10. Even Kuhn himself seems to be sufficiently under the allure of the classical model to describe the acceptance of a paradigm as including an "arbitrary" element (Kuhn, 1970: 4-5). Eventually, though, Brown comes to the conclusion that Kuhn, as he himself, opts for the conclusion that the classical model of rationality has to be *replaced* (1988: 112).

11. I find Brown's call for a *replacement* of the classical model too severe. That model does represent - as far as it is applicable - important insights about rationality. It has to be *complemented* by a model which also provides for the rationality of judgements.

12. "Science is not the only field in which rationality is possible..."(Brown, 1988: 207)

13. For an excellent elucidation of the textual character of the object of the human sciences, see Ricoeur, 1981: 145-164 and 197-221.

14. Cf. the following statement: "If, to take a wild example, all of the physicists in the world were to agree that, beginning today, data would be communally fudged, and results announced would be those they had agreed to announce, we would have communal decisions by the relevant experts, and no qualified dissenters, but these decisions would not result from the exercise of expertise, and would not be rational" (Brown, 1988: 196). If one takes the way Brown understands rationality seriously, it escapes me completely why such decisions ought not to be regarded as rational.

15. The importance of modes of communication for other developments in the culture of a society is also emphasised by Alvin Gouldner (1976).

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12

On presuppositions and the historical study of the Jewishness of Jesus

Willem S. Vorster

One of the most difficult, but also most interesting problems of New Testament research concerns the construction/reconstruction of the historical Jesus. This is an unsolved problem which has been in focus for the past few centuries. The first phase of this research, known as the "Old Quest", was concluded by the monumental work of the renowned Albert Schweitzer who wrote a critical history of the life of Jesus research since Reimarus to Wrede, that is from the late eighteenth to the early twentieth century (cf Schweitzer 1966). This work put an end to the idea that it was possible to write a life of Jesus. For almost half a century it was accepted that due to the lack of sources and the character of the extant sources it was no longer possible to know with certainty who Jesus really was or to write a biography of Jesus. In the words of Bultmann (1958: 14):

> I do indeed think that we can now know almost nothing concerning the life and personality of Jesus, since the early Christian sources show no interest in either, are moreover fragmentary and often legendary, and other sources about Jesus do not exist.

It was only in 1953 that the view was challenged when a "New Quest" for the historical Jesus was started with an address of Käsemann, a former student of Bultmann, at a meeting of the "Old Marburgers" (cf. Käsemann, 1954). Gradually consensus started growing about the future of historical Jesus research, but scholars now realised how difficult it was to derive with certainty deeds and words of Jesus from the sources at their disposal. Optimism about the possibility of reconstructing the historical Jesus and the necessity of this research persists, despite many difficulties and the impression of "so many men, so many minds". Of special interest for the purpose of this essay is the fact that

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during the past two decades there has been a growing interest in the relationship between Jesus and first-century Judaism, or to phrase it differently in the Jewishness of Jesus. One of the remarkable features of this research is how the results of these studies differ and how many different pictures are presented of Jesus the Jew.

In a recent article on the subject, D.J. Harrington (1987a: 36) draws attention to "seven different images of Jesus that have been proposed by scholars in recent years, the differences relating to the different Jewish backgrounds against which they have chosen to locate their image of the historical Jesus". These images include Jesus the Jew being any of the following: an eschatological prophet, a political revolutionary, a magician, a Hillelite or proto-Pharisee, an Essene, a Galilean charismatic or a Galilean rabbi. These images are the result of Christian scholarship. In addition there are the different Jewish views of Jesus which add to or complement the variety of views held about the Jewishness of Jesus (cf. Batdorf, 1984: 190-191).

The discovery of the Dead Sea Scrolls, the renewed interest in the Old Testament Pseudepigrapha, the growing awareness that Jews had been part of the Hellenistic world since the adventures of Alexander the Great, an ongoing reassessment of the rabbinic writings have all contributed to our knowledge of first-century Palestinian Judaism, and according to Harrington (1987a: 33-36, see also 1987b) this in turn explains the resurgent concern about the Jewishness of Jesus. It should also be taken into account that much of the research is taking place in the context of the post-holocaust debate about the Jewishness of Jesus, which explains some of the interest in the topic. In this regard it is interesting to compare, for example, the ecumenically minded Professor David Flusser's views with those of his predecessors in the debate about Jesus the Jew.

The results of historical Jesus research are moreover complicated by the "hermeneutic matrix" (Batdorf, 1984: 187) underlying the research. The focus on particular aspects of Jesus, for example, "... Jesus as authentic person, as prophet, as Jew, as Messiah, as liberator, as story-teller, and as magician" (cf. Batdorf, 1984), and the application of a variety of methods in this research also explain the current dilemma with which a student of the historical Jesus is confronted. The shift in focus from Jesus as the eschatological apocalyptic prophet, which has dominated the research for the last century, to Jesus the Jewish wisdom teacher which is currently taking place in certain circles (cf. Downing, 1987 and Mack, 1988), further complicates the picture.

The application of a variety of methods with the resultant differences in interpretations offered, the different perspectives on the problem and the ongoing attempts to interpret Jesus of Nazareth historically are healthy developments in historical research and should all be welcomed. It should also urge New Testament scholars to come to grips with the problem of the validity of their scientific work. Although it is clear that there is no such thing as one objective interpretation of the Jewishness of Jesus, it is also time to ask whether Jesus the Jew in such a variety of roles is indeed necessary and valid.

A starting point would be to develop an awareness of the role of presuppositions in these interpretations. This would, however, mean that greater attention needs to be paid to the nature and variety of presuppositions that play a role in interpretation. The next part of my essay will deal with the phenomenon of presuppositions with regard to the study of the historical Jesus. The third part will offer a short case study on some presuppositions in three recent studies on the Jewishness of Jesus.

It is commonly accepted by New Testament scholars that presuppositionless interpretation is impossible. Since the famous essay by Rudolf Bultmann (1957. Cf. also Palmer, 1969: 135-136; 182-184) on the matter, there has developed a general awareness among scholars that every interpretation is based upon presuppositions, and that interpreters should be willing to reflect on the role of presuppositions in their scholarly work. This does not mean that interpreters of the New Testament are explicit about their presuppositions, or that the nature and character of presuppositions are often discussed in these circles. Since New Testament scholarship concerns the study of ancient, religious texts which have been canonised, a great variety of presuppositions is involved in the interpretation of these texts. These presuppositions are not different from those involved in other historical or literary disciplines, or for that matter in scientific research as such. They are of a philosophical, theological, historical, methodological, literary and epistemological nature. But, what are presuppositions? Are they to be regarded negatively? Can, and should they be controlled?

Interpreters never approach their objects of interpretation empty-headed, that is, without a preconceived idea of their object. Hodgson (1979: 136) correctly observes that "Science exists in the minds of men living in a human society, and that the ideas and beliefs and concepts of the time inevitably set their limits on science". We have seen above how the basic assumptions on the possibility of reconstructing the historical Jesus have shifted through the times. Assumptions of this nature influence interpreters consciously or subconsciously and are normally taken for granted because they belong to the domain assumptions and data beliefs of a particular scientific circle (cf. Botha, 1984). Only when these assumptions are questioned or approached from a different frame of mind are they challenged, validated or rejected. We have also noticed that increasing knowledge about first-century Judaism and a renewed interest in the Pseudepigrapha are given as reasons why images of the Jewishness of Jesus are changing. It makes quite a difference, for instance, whether an interpreter of the Jewishness of Jesus assumes that Jesus was a member of a society with rigid views on the fulfilment of the law or whether one accepts that there were different religious groups in first-century Judaism each with its own interpretation of the law. This is just another example of how pre-understanding forms the background against which scientific, historical judgements are made. A few remarks about prejudice and pre-understanding will further illustrate the matter.

Stanton (1977: 61) draws attention to the fact that "presupposition" and "prejudice" are often used loosely as synonyms. Personal factors like optimism or pessimism may well give rise to different interpretations, he argues. Cultural factors also play their role. For the oppressed, for instance, Jesus the Jew is someone different from the Jesus of the oppressor. Wittingly or unwittingly these factors tend to condition interpreters into a biased direction. This is illustrated in Christian art where Jesus is pictured in so many ways because of this type of influence. Since (social) reality is a construction of the mind (cf. Berger and Luckmann, 1967) the interpreter is influenced by his/her conception of his/her social reality in constructing an image of Jesus. This is clear from the different images we find of Jesus in the Gospels of the New Testament, but also from past and current images created by interpreters of the New Testament as we have seen above.

At this stage it is also important to draw attention to the role which faith commitments and dogmatic assumptions can play as forms of prejudice. Perhaps this is best seen in theological predispositions concerning Christology. Depending on the event in the life of Jesus on which the emphasis is placed, a different Christology is developed. Those who believe that the incarnation of Christ is the most important aspect of his life have a totally different conception of who Jesus was than those who give priority to his resurrection (cf. Bosch 1986: 1-8). These conceptions soon become beliefs and convictions which influence interpreters to make biased interpretations of texts. New Testament scholars normally try to reject the validity of dogmatic and faith assumptions although it is not always possible to avoid them. In fact, "W. Wrede saw the history of New Testament scholarship in the eighteenth and nineteenth centuries as the constant struggle of historical research to cut itself loose from dogmatic prejudgements" (Stanton, 1977: 62). However difficult it might be, the New Testament interpreter should rid him/herself of this kind of historical manner where there is no place for dogmatic assumption and prejudice.

In the article mentioned above Bultmann (1965: 146) furthermore makes the valid point that the understanding of a text presupposes a pre-understanding of the subject matter. Someone who interprets economic history is supposed to have a pre-understanding of economics and society, and the same goes for the interpreter of religion or philosophy. Without a knowledge of philosophy it is impossible to understand philosophy. In similar manner the New Testament historian has a pre-understanding of not only his sources, but also of historical investigation, the preaching of Jesus, semantics and so on. Pre-understanding is, however, not understanding; it simply offers the possibility of understanding. That is why it is so important for the interpreter of a text to have his/her pre-understanding checked and controlled by the text.

The interpreter's views are moreover conditioned by presuppositions, that is assumptions which are taken for granted, or assumptions that involve a necessary or contingent truth (cf. Flew, 1984: 286). A few words on domain assumptions and data beliefs, being basic presuppositions in the construction of the Jewishness of Jesus, are necessary.

According to Radnitzky (1974: 1), "The scientist views his subject matter in a certain perspective, and this has a decisive influence on the direction of his research programme ...." This also holds good for the New Testament scholar who has certain preconceptions about the subject matter of his/her discipline. He/she views the territory of the subject from a certain perspective, and it is this perspective that determines the direction of his/her research. In the historical critical paradigm, which dominated New Testament research during the past century, the New Testament scholar is seen as a highly qualified historian who studies his object, the New Testament as product of a first-century religion, objectively. His/her task is to interpret and understand the newly founded religion and its literary legacy with the aid of historico-critical methods. Thus the task is an historical and a literary one. It includes research on the history of early Christianity, what these people thought and believed, and the message of the literary documents which was their legacy. The sources for this study are archaeological artefacts, knowledge of the world in which the religion was founded, and the writings of the New Testament. These writings are normally regarded as windows through which Jesus and the first Christians, their beliefs, and convictions and deeds can be seen.

Within this disciplinary matrix it is furthermore assumed that the hub of early Christian religion was the cult and liturgy. Religious convictions were transmitted by folklore and oral communications which later were written down. These writings, the New Testament documents, are not the products of literary activity. They are the result of missionary and cult activities. This explains why the documents should be studied historically.

These basic domain assumptions determine both the perception from which New Testament scholars study the material and how it should be studied. Knowledge is obtained by the careful dismantling of texts in order to discover what lies behind these texts and the "reconstruction" of historical persons and their deeds. The purpose of the study is to explain and understand the growth of texts and beliefs (cf. Jeanrond, 1986: 12ff). In the case of the historical study of the Jewishness of Jesus the basic epistemological presupposition since the start of the "New Quest" is the fact that, however difficult it may be, we have the tools to recover the main events in the life of Jesus which include his words and deeds.

In spite of new developments in recent New Testament scholarship with regard to text theories and the processes of interpretation, the application of modern concepts of text, literary and sociological theories, it would be fair to say that the historico-critical matrix forms the general background and the domain assumptions against which the historical Jesus research is taking place. It is assumed that historical criticism, as understood by New Testament scholars provides the means and the tools to get to know the Jewishness of Jesus.

Another set of presuppositions which determine the research programme and eventually the results are the beliefs about the data which is studied. The main sources for the study of the historical Jesus are the New Testament writings and especially the Gospels, including the Gospel of John. When studying the sayings of Jesus extracanonical sources such as the Gospel of Thomas are also taken into account. Because of the very nature of these writings the assumption has grown that it is no longer possible to say anything for certain about the historical figure of Jesus. The differences but also the similarities between the different Gospels even when given in detail about what Jesus would have done and said, when and where and why he would have done what or said what for what purpose, make it impossible to read his life story and what he stood for from the surface of these texts. The possible data concerning his life, deeds and words have to be sought for in the layers of tradition which precede the written phase of these writings. The data for the construction of an image of Jesus the Jew has to be inferred from these sources. In the case of the sayings of Jesus the data is carefully reconstructed in order to determine the authentic sayings of Jesus, their exact wording, that is what he actually said *(ipsissima verba)*, or to attempt to come as close as possible to what he would have said *(ipsissima vox)*. In short, the data concerning the deeds and the words of Jesus has to be reconstructed from very few extant sources which were not written for that purpose. For this reason every individual source also has to be judged for its historical value. This may seem a very sceptical approach to sources, but the character of the sources makes such a rigorous approach necessary.

New Testament scholars are in general aware of the difficulties in establishing the authentic sayings of Jesus and about the hypothetical nature of the undertaking. Even in the case of the parables, which are commonly regarded as the core of the teaching of Jesus, it is very difficult to determine the "original" version of any particular parable. Nevertheless it is regarded as "possible". The matter is, however, complicated by the fact that it is apparently assumed that if it is possible to reconstruct the original wording of a parable the meaning of the parable will also be clear. The problem is that the reconstructed text is a contextless text, because by no means is it possible to reconstruct the original context of communication! The same goes for the rest of the sayings material since it is clear from the Gospels that many sayings of Jesus were re-applied in contexts other than those of the original communication for different purposes. The point is that data assumptions condition interpreters to make specific interpretations in view of these assumptions.

Methodological presuppositions play an important role in historical interpretation. It is, for example, assumed that by the application of form critical and tradition critical criteria one can recover deeds and words of Jesus. This again is based on the view of how the relevant texts came about and what texts are. The methods applied in the study of the Jewishness of Jesus very often determine the outcome of the study (cf. Crossan, 1986) as is clear in the studies on the sayings of Jesus. Obviously the historical interpretation of Jesus the Jew concerns more than the reconstruction of the sayings of Jesus and therefore other methods are also used to make historical judgements in this connection. These also depend on presuppositions as we shall see below.

Other presuppositions which play a role in the historical study of the Jewishness are language presuppositions. It is, for instance, assumed that by analysing the words and sentences in the Greek New Testament, it would be possible to determine the message of Jesus. Especially in circles where a traditional philological approach to the study of the use of language in the New Testament is taken, there is little awareness of the complexities of language and semantics. As soon as modern linguistic insights, like speech acts and metaphoricity are taken into account another set of presuppositions is introduced and these greatly influence the image of what Jesus had said and why this was said in a particular way. The teaching of Jesus on the kingdom is perhaps one of the best examples of how views on apocalyptic eschatology precondition us. In the discussion of Riches's book below I will return to the role of language presuppositions in the historical study of the Jewishness of Jesus.

Very important to our discussion of presuppositions with regard to historical study are those concerning historical interpretation. There are many theories about historical interpretation and this is not the place to discuss them (cf. Vorster, 1984). Obviously New Testament scholars also have their theories about historical interpretation. Let me mention a few presuppositions in this regard. It is already clear from the domain assumptions and data beliefs discussed above that, in some circles at least, there are those who view historical interpretation as a relationship between a knowing subject and a knowable object. A New Testament historian studies an historical object (cf. however Bultmann, 1960: 142ff). His task is seen as one of reconstruction. In other words the historical event, word or deed has to be reconstructed in the sense of reality remade. The point is, however, that no historical interpretation can claim to be a reflection of what really happened. By their very nature historical judgements are not objective descriptions of what really happened. They are socially conditioned constructions of the past (cf. Mommsen, 1978: 19-35). They are products of the mind build on presuppositions and perceptions of a great variety. That is why I have used the traditional term "reconstruction" and the term "construction" to indicate the theoretical differences and presuppositions underlying both terms. Sources are only the starting point in historical study. The historian works with more than "bare facts" like dates, events, persons and so on. He/she constructs the past by relating things and persons from the most probable perspective. This relationship depends on different presuppositions.

If it is thought that there is a cause-effect relationship between events and that the task of the historian is to analyse causes and effects, one will interpret the life and death of Jesus from that perspective. It has, however, become clear from studies by B.F. Meyer (1979) on *The aims of Jesus* and A.E. Harvey (1982) on *Jesus and the constraints of history*, that the relationship between events is complex and that these relationships can be interpreted in different ways.

The "historical Jesus" is the product of historical judgements (cf. Robinson, 1983: 26-32) which depend on many and different presuppositions. This is not to be regarded as something negative. It is characteristic of the nature of knowledge (cf. Cornelis, 1973: 163ff). One can compare history with a written text. Texts do not have meaning. Meaning is something which the interpreter ascribes to a text on the ground of an interpretation which is based on different presuppositions. Historians also ascribe meaning to history and that explains why there are different images of the same person or event. The historian plots his/her story in the light of perceptions and presuppositions conditioned by knowledge and context. The only way to control presuppositions is to articulate them and to have them put to the test by other interpreters of the same material. In such a manner vantage points can be judged.

In the next part of the essay I shall focus on certain presuppositions in three recent studies of Jesus and Judaism. Because of the lack of space I shall focus on single aspects of each study. The purpose is to discuss presuppositions in use.

It is not practice for scholars who study the Jewishness of Jesus to bring the presuppositions on which their interpretations are based out into the open. These have to be studied by close reading and by deriving presuppositions from statements being made and results that are reached. I shall take three recent publications and concentrate on single aspects concerning presuppositions in each.

Our first case study concerns the works of G. Vermes, a Jewish scholar from Oxford, and an authority on Intertestamental Jewish literature and first-century Palestine. I have included the following of his works for our discussion on presuppositions with the emphasis on the first:

Vermes, G. 1973. *Jesus the Jew: A historian's reading of the Gospels*. London: Collins.
1983. *Jesus and the world of Judaism*. London: SCM.
1985. Methodology in the study of Jewish literature in the Graeco-Roman period. *JJS* 36, 145-158.

In view of the subtitle of the 1973 publication by Vermes and the fact that he is an authority on first-century Judaism and contemporary Jewish literature, I shall pay attention only to presuppositions concerning his views of history and historical interpretation. Vermes is not explicit about his presuppositions concerning historical interpretation. These are, however, clear from statements he makes concerning his aim, his data beliefs and the material he treats in his study of Jesus the Jew.

He starts his book on Jesus the Jew (1973: 15) with a quotation from the Nicene creed in order to indicate the difference between "... history and theology, fact and interpretation". According to him his book is a historical interpretation of "historical facts". His interest lies in history and the "... *historical* significance of words and events recorded in the Gospels. What they are *believed* to signify is the business of the theologian; the historian's task is to discover the original meaning of their message" (1973: 16). His study is "... prompted by a single-minded and devout search for fact and reality and undertaken out of feeling for the tragedy of Jesus of Nazareth. If, after working his way through the book, the reader recognizes that this man, so distorted by Christian and Jewish myth alike, was *in fact* neither the Christ of the Church, nor the apostate and bogey-man of Jewish popular tradition ..." (1973: 17; cf. also Vermes, 1983: 1, 25). In order to achieve this aim he sees his task in reading the material against the background "... of the literary legacy of Palestinian and Diaspora Jews from the last two hundred years of the pre-Christian, and the first centuries of the Christian era ..." (1973: 16). These statements reveal Vermes's presuppositions about history, historiography, "fact" and interpretation and his attitude toward the subject matter. He does the study because of a personal interest and aims to rectify a distorted picture and because of his interest in historical interpretation, which he regards as objective.

Vermes's 1973 publication on the Jewishness of Jesus consists of three chapters on the life of Jesus followed by a much longer second part comprising five chapters on the titles applied to Jesus in the Gospels. These include Jesus the prophet, the lord, the Messiah, the son of man and the son of God. The conclusion is that Jesus was a noble character and profound teacher, one of the Galilean holy men *(hasidim)* of his time.

The first thing to be noted about the presuppositions of Vermes concerning historical interpretation is that the "historical facts" of the life of Jesus the Jew are derived mainly from an analysis of the names of Jesus. Although the first chapter of the book offers a skeletal life of Jesus based on the Synoptic Gospels, the major part of the book is devoted to the titles given to Jesus (chapters 5-8). This is surprising since the names of Jesus in many respects reflect the ideological points of view of the authors of the Gospels, and perhaps are the best indication of how he was seen and not who he "really" was. It is furthermore surprising that Vermes never pays attention to the historical value of his sources, the Synoptic Gospels. He does not even raise the question of the sources of the Synoptic Gospels and their transmission in chapter one where he presents a skeletal life of Jesus. If his purpose was to offer an image of Jesus the Jew, based on "historical facts" as he asserts, one would expect to find the critical awareness of an historian concerning the historical value of his/her sources (cf. also Horbury, 1974: 228). Furthermore one would expect a more comprehensive approach to the "facts" in the life of Jesus. This approach by Vermes reveals his presuppositions on data as well as how he sees "reality".

The heart of the book is in many respects chapter three. Drawing on earlier studies on miracle-workers in Galilee and his knowledge about Jewish sources in the time of Jesus, Vermes compares Jesus with people like Honi and Hanina ben Dosa. Both of them were holy men living in Galilee during the first century C.E. Like Jesus they are regarded as Jewish charismatics, miracle-workers and heirs of "... an age old prophetic religious line" (1973: 69). According to rabbinic sources they played an important role in Palestinian religious life during the final period of the Second Temple era (cf. Vermes, 1973: 69ff). This is the filter through which Jesus is seen by Vermes and the angle from which he constructs an image of Jesus.

The focus on Jesus as one of the *hasidim* of first-century Judaism Vermes regards as hypothetical, but nevertheless an hypothesis which has a high degree of probability. He maintains (1973: 78f):

> For all the precautions to a thesis which, owing to the nature of the sources, must in part remain hypothetical, coincidence cannot reasonably be invoked to account for all similarities ... It would appear, rather, that the logical inference must be that the person of Jesus is to be seen as part of first-century charismatic Judaism and as the paramount example of the early Hasidim or Devout.

Since it is probable that Jesus regarded himself as prophet, and most of his contemporaries would have seen him as one of the prophets, and prophet in the Gospels also means miracle worker, for example, Vermes finds enough evidence in the "facts" to corroborate his view on Jesus the Jew. Be this as it may, Vermes has made a specific choice for a particular type of charismatic, and has made that the lens through which he interprets the "facts". In other words, he has constructed an image of Jesus by "facts" within that background.

211

With regard to the relationship between "facts" and "interpretation" and Vermes's views on the matter, this becomes interesting.

Vermes wants to give objective history, not interpretation. "Facts" are in other words in his view things which can be known without mediation, that is without a theory of perception. In his own treatment of the "facts" he, however, uses the prophetic, charismatic background of first-century Judaism to construct his image of Jesus. The point is of course that even in Vermes's construction "facts" are interpreted facts and not "bare facts". "Reality" is reality remade by the interpreter, also in the case of historical interpretation.

Our next case is the book by John Riches, *Jesus and the transformation of Judaism*, published in 1980. The study is chosen to elucidate how views on the use of language forms the basis of interpreting the intention of Jesus. In his introduction the author reveals the background to his interest in the purpose of Jesus. While teaching New Testament at Glasgow he also worked in a church in a downtown area. "Questions about the relation of Christian preaching and theological utterance to social and political attitudes and actions are obviously pressing in such areas and I hope that at least indirectly this study may help to shed light on them" (ix).

Riches takes up the question about the purpose of Jesus originally raised by Reimarus in the eighteenth century. "What, as a man belonging to a particular community with a particular history, aspirations, etc., was he about?" To answer this question adequately Riches argues that the following two questions also have to be answered: How is it possible to say new things in a particular given language? This question regards the ways in which Jesus took religious vocabulary of his day and reworked it for his own purposes. The second question concerns the relation between theology and its social, political, "attitudinal" implications. "If we are to see Jesus as a man of his time, then we have to know how his fairly explicitly religious theological utterances related to the broad range of social and political questions which concerned Jews of the first century" (viii).

Riches's basic presupposition is that a study of the use of language by Jesus and the relation between his uttered beliefs and his practice can enable us to determine the intention or purpose of Jesus (cf. Riches, 1980: 11-12). He builds his thesis on the data beliefs of the History of Religions School and critical research of the past century on the sayings of Jesus. Drawing on the work by Troeltsch, for example, Riches believes that "... the fundamental problem for a thoroughgoing historical explanation of Jesus' teaching is not simply how to plot the relationship between his teaching and the beliefs of contemporary Judaism, but rather how to understand the religious beliefs at all ..." (15). According to Riches religious utterances should be studied in relation to the changing circumstances of a particular community. He maintains that it would be possible to determine the intention of Jesus if traditional associations of the

212

language he used were determined and the significance of what he said was established in the new situations in which his beliefs were uttered. Since words have conventional associations in a particular community at a certain time, it would be necessary for someone who wishes to enunciate new truths in that particular society to change some of the conventions and associations. If Jesus, for example, wished to say something new with the sentence "The Kingdom of God has come", he would have had to change some of the conventional associations of the sentence (cf. Riches, 1980: 36-37). The actual use of terms enables one to determine which conventions a particular speaker is challenging.

Understanding the language of Jesus is instrumental in understanding his religion and his intentions according to Riches. But, how does one go from the use of language to the intentions of the user? Riches argues that it is possible to interpret what people say on the basis of what they *utter*. Although he does not use the terms "pragmatics" or "speech acts" it is clear that his interest lies with what people do with their words. In other words, he subscribes to the insights of modern linguistics that people do things with words and that it is not enough to investigate the meaning of sentences only. However, the function of sentences also has to be studied because the same sentence can be used to do different things, and therefore for different meanings. For that purpose, Riches distinguishes between sentences and utterances. He maintains that "... sentences, terms and other sorts of expression, are repeatable types which may be used by different people at different times" (cf. Millar & Riches, 1981: 30) while utterances are intentional. "In the very broader sense, *what an utterance means* is what the speaker intended in making it" (Millar & Riches, 1981: 80, also Riches 1980: 29ff). This is the very basic presupposition of Riches's book on Jesus and his contribution to the historical study of Jesus. In chapter five of his book he analyses the preaching of Jesus of the Kingdom of God to illustrate that Jesus' message "... sets him clearly in the ranks of those who sought a renewal and restoration of Judaism" (Riches 1980: 87). The rest of his book deals with Jesus and the law of purity, his theism and his role in the transformation of first-century Judaism. Let us have a brief look at aspects of the chapter on the "Kingdom".

After a discussion of traditional usages of Kingdom of God terminology in first-century Palestinian Judaism, that is by the Zealots, the Qumran community, the Pharisees, the Essenes and John the Baptist, Riches argues that Jesus retained the "core meaning" of the term, but changed its associations by using it in non-conventional contexts of communication (cf. Riches, 1980: 93-111). These contexts refer to his sharing meals with tax collectors and sinners, his healings and exorcisms, his call to discipleship which enabled him to strip the term of its conventional associations of purity and to replace them with others. "In this he succeeded in deleting its militaristic and ritual associations and bringing it into association with notions of forgiveness and healing, of joy and service" (Riches, 1980: 100). In other words, although Jesus used conventional kingdom terminology, the sense of the term and also of the utterances in which it was used, was changed radically.

The data beliefs of Riches are clear. We can know the intention and purpose of Jesus and his beliefs by studying the sense of his utterances. He furthermore assumes that it is possible to determine the intention of an author (speaker), in our case of Jesus, by analysing the sense of his utterances. Historical interpretation is therefore made on the ground of the study of speech acts in the sayings tradition of Jesus within the context of first-century Judaism.

In addition to the difficulties in determining the authenticity of sayings of Jesus, of which Riches is well aware, the question arises whether it is possible to determine the intention of an author by studying the texts he uttered. Furthermore, is it at all possible to determine the sense of an utterance without knowing the original context of communication? How is it possible to construct original contexts of communication for the sayings of Jesus? These are a few questions which arise on the ground of the presupposition concerning the sense of utterances in the work of Riches (cf. Freundlieb, 1983).

Our last case concerns the book of E.P. Sanders, *Jesus and Judaism*. This book, written by an authority of first-century Judaism and Oxford professor was published in 1985. Like Riches, Sanders is interested in Jesus' intention and his relationship to his contemporaries. Unlike Riches, Sanders does not study the subject of his inquiry by analysing the sense of his utterances, but by interpreting certain assured "facts" about the life of Jesus historically and using his utterances only secondarily in interpreting the facts. Being rather sceptical about our ability to determine the authenticity of the sayings material in whole or in part (cf. Sanders, 1985: 4), he prefers not to study the intention of Jesus via the sayings material. Instead he prefers to do it by establishing the facts about Jesus which will explain why Jesus was in conflict with the priesthood in Jerusalem, why he died at the hands of the Romans and why an eschatological movement was initiated in primitive Christianity. The image he offers us of Jesus the Jew is that he was deeply involved in the expectations of first-century Jewish restoration eschatology, and a man of own convictions and mind.

For the purpose of this essay I will concentrate on the data assumptions of Sanders in view of the fact that they are unconventional in terms of current approaches to the Jewishness of Jesus. A few general observations are, however, necessary before we come to this.

The book offers the insights of an authority on both the Jesus traditions in the New Testament and Judaism. Riches (1986: 53) correctly observes that "The beliefs and expectations of first-century Jewish renewal movements are copiously documented and the book is a mine of information and full of fresh insights." Sanders is a good historian who is willing to articulate his assumptions about historical interpretation of Jesus, about the nature of historical judgements, and about the method he adopts. He maintains that "The method which ... seems necessary to follow in writing about Jesus, is to construct hypotheses which, on the one hand, do rest on material generally considered reliable without, on the other hand, being totally dependent on the authenticity of any given pericope" (Sanders 1985: 3). And furthermore, "The full reconstruction to be sure, involves inference, surmise and conjecture" (321). Historical judgements have different degrees of certainty in his interpretation on the following scale: Beyond reasonable doubt, highly probable, probable, possible, conceivable, unprovable, incredible (320). A major presupposition is that "... we can pretty well know what Jesus was out to accomplish, that we can know a lot about what he said, and that those two things make sense within the world of first-century Judaism" (2). Let us turn to his presupposition concerning the facts about Jesus.

Sanders (1985: 11) regards the following facts as almost indisputable: 1. Jesus was baptized by John the Baptist; 2. He was a Galilean who preached and healed; 3. He called disciples and spoke of there being twelve; 4. He confined his activity to Israel; 5. He engaged in a controversy about the temple; 6. He was crucified outside Jerusalem by Jewish authorities; 7. After his death his followers continued as an identifiable movement; 8. At least some Jews persecuted parts of the new movement, and it appears that this persecution endured at least to a time near the end of his career. These "facts" are put into a framework of understanding conceived by him as the most probable context of knowing and understanding. He thoroughly locates these facts into the context of Jewish hopes for the restoration of Israel and develops an image of Jesus. The implication is of course that Sanders is no longer working with the "bare facts" in his construction of an image of Jesus. He works with his interpretation of the facts. This is very well illustrated by his interpretation of the cleansing of the temple. He forcefully argues (75) a case against the traditional interpretation and maintains that:

> Jesus predicted (or threatened) the destruction of the temple and carried out an action symbolic of its destruction by demonstrating against the performance of the sacrifices. He did not wish to purify the temple, either of dishonest trading or of trading in contrast to "pure" worship. Nor was he opposed to the temple sacrifices which God commanded to Israel. He intended, rather, to indicate that the end was at hand and that the temple would be destroyed, so that the new and perfect temple might arise.

This thesis is clearly Sanders's interpretation of the indisputable fact: Jesus engaged in a controversy about the temple. I use this quotation to illustrate the implications of Sanders's presupposition concerning the data on which he presumably bases his construction of Jesus the Jew. The point is, that facts are never bare. We do not encounter facts without the mediation of some theory of interpretation or perception. Riches (1986: 53) is correct in saying that "... facts allow for different modes of description", and one can add, different interpretations. Interpreters ascribe meanings to facts as they ascribe meaning to written and spoken texts. Whether one agrees with the interpretation of Sanders or not is not the point I am making. The point is that even though Sanders regards some facts about Jesus as "indisputable", they still remain building blocks in the construction of images which can be shaped by the interpreter.

The three case studies briefly illustrate a small aspect of the presuppositions involved in the historical study of the Jewishness of Jesus.

It is necessary in conclusion to make a few remarks on what has been presented in this essay. First of all it seems clear that presuppositions of a great variety influence us in doing historical research on Jesus the Jew. The three case studies illustrate that presuppositions concerning the data which are used in constructing an image of Jesus thoroughly influence interpreters and also explain why interpreters with the same purpose in mind often construct different images. Secondly it has also become clear that scholars have to be conscious about presuppositions and that it would be very helpful if they were willing to articulate their presuppositions. It would help interpreters to judge the validity of presuppositions and the influence they have on interpretation. An awareness of presuppositions and their role in interpretation and discussion about presuppositions might lead to progress in our search for images of who Jesus was and what he intended to do. Lastly, I hope that it has become clear how difficult it is to study the presuppositions on which scholars base interpretations when they are not made explicit, and how urgent it is to develop a set of criteria to judge the validity of presuppositions in historical study of the New Testament.

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218

Morality and ethics in the practice of social research

13

State censorship, research in the human sciences and library resources in South Africa

Christopher Merrett

The nature of censorship in South Africa

It is generally accepted by both proponents and opponents that censorship is a massive institution of far-reaching influence and importance in South African society. In view of its size it is generally analysed in terms of its constituent parts, an approach which contains the danger of losing the broad picture in a welter of specific detail. Similarly we carry our own stereotypical images of censorship, no doubt suggested by our individual concerns and interests: books in locked cupboards; people in locked cells; the suspension or outright banning of anti-apartheid newspapers; and the exclusion of reporters and photographers from the scene of unrest. All are equally valid, along with many others, and they exemplify the need for caution in this field. First of all, the institution as a whole must not be forgotten and it should always form a backdrop to a discussion of its components. Secondly, it is important to be clear about the components in question. Thirdly, their analysis in isolation is helpful in assessing the tactics of the moment, for while the strategy of censorship has remained broadly consistent, the specific means employed to pursue it have varied with circumstances.

Censorship is an act of political control commonly employed through space and time by governments with authoritarian tendencies. As an act of violence against cerebral and intellectual activity its use suggests insecurity and uncertainty, or even fear, in the minds of those who employ it. It is of course the antithesis of academic freedom which values free discourse and the pursuit of argument in an atmosphere of mutual respect. Similarly it denies basic human rights and dignity in its assumption that individuals cannot perceive flawed and fraudulent arguments. In the latter instance censorship adopts a protective and even avuncular mien but this is generally euphemistic: even apparently well-

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intentioned censorship generally results in the fostering of lies and evasions and the submerging of the truth.

State censorship is prevalent in all societies, including those which consider themselves to be bastions of democracy: in the United Kingdom, for example, the government has recently prohibited Sinn Fein from access to channels of communication. In South Africa, a country in which it is government policy to stress the differences between people rather than their common humanity; in which the mass of the people are denied access to a participative role in the shaping of society of which they are a part; and in which the ruling elite is prepared to accept civil war as a price for maintaining the status quo, censorship is a logical necessity. It is certainly not an aberration or an accident of history and it will not disappear until there is fundamental change in the political economy. At this particular moment it is an especially obvious characteristic of South African life with an authoritarian ruling elite attempting to reform a strategy of four decades. It is simultaneously failing to satisfy both opponents seeking more profound change; and its own former adherents of continuing antediluvian persuasion. The South African State, which has for decades required censorship to underpin its social engineering programme, is now in greater need of it than ever before. Thus in the late 1980s it manifests itself in an increasing number of ways.

Types of censorship

It is possible to reduce the South African censorship system to a six-tiered model. Two of these layers will form the subject of this paper, but as already emphasized they cannot be considered in isolation of the others. The broadest tier is apartheid itself which in the opinion of some (Gordimer, 1972) is the greatest censor of them all. It keeps the mass of South Africans at a significant distance from the means to communicate and without the political power to change their socio-economic circumstances. Secondly, and intimately linked with apartheid, is education which has well-known ideological dimensions. Black education, as provided by the State, has inhibited socio-political aspirations and controlled economic utility. Its inferiority can be shown quantitatively in the amounts spent on black and white children;[1] and qualitatively in the frequency with which the education issue has provided a catalyst for unrest. Inferior education inhibits the development of communications skills and structured thought and adds to the frustration of a clearly perceived second class status in society. This educational censorship has had a devastating effect on communication between different groups of South Africans and frequently promoted a state of mutual incomprehension.

Thirdly, many outstanding individuals, black and white, have evaded the institutionalized censorship of apartheid and its educational system. In order to control dissident opinion the government has created an impressive array of often overlapping security legislation to control the individual and corporate right to communicate. It owes its origins to the Suppression of Communism Act of 1950 and its supplements and amendments of the following three decades which were drawn together in the Internal Security Act of 1982. Security legislation has allowed for the listing and banning of individuals thus destroying their right to communicate and to circulate freely; the detention without trial of others which has had the same effects; the banning and seizure of assets of organizations representing significant viewpoints within South African society; and the proscription of newspapers and journals such as the *World* and *Pro veritate*. These measures have eliminated from the political and social process the free exchange of views and information from crucial areas of the South African polity. They are also of course blatantly intimidatory and are clearly designed to impress those people and organizations which feel they might be targets in future. Such a climate of tension and uncertainty is a fertile breeding ground for the aim of all censors: the self-censored society. In such a scenario the censor can fade into the background and claim that society is free in the knowledge that opponents have been intimidated into silence. In South Africa today part of that path has already been travelled with devastating effects on research and learning.

The fact that the authorities have failed to achieve quiescence of the critical is shown by the growth of the security State exemplified by the State of Emergency currently in force. The enabling legislation is contained in the Public Safety Act of 1953 and the first Emergency was declared in 1960. Twenty five years later, on 21 July 1985, a second partial Emergency was declared, mainly in the Cape and Transvaal. It was lifted in March the following year and after a respite of three months a full Emergency, the third, was declared on 12 June 1986. Its successors, the fourth and fifth Emergencies, have ensured that South Africa has remained in what appears to be a permanent State of Emergency. Its purpose may be reduced to a number of tactics which together form a 'total strategy', pushing forward the government's modernization of apartheid: to eliminate the popularity and effectiveness of the extra-Parliamentary opposition; to buy the compliance of the majority of the black community; and to put in place moderate and pliable local government structures. The successful prosecution of this strategy requires an unprecedented level of censorship to prevent the circulation of data - inside and outside the country - of the means used by the authorities to suppress the challenge to the State which has been endemic since the Vaal Uprising of September 1984.

Closely allied to the censorship implications of the State of Emergency is a fifth tier which inhibits access to data about the South African polity and the actions of its government. This is a familiar type of censorship on the international scale but in South Africa it is particularly well developed. In general it is true to say that in-depth research on the police, prisons, oil supply, uranium, nuclear energy, defence, post mortem proceedings, national supplies procurement and national keypoints is impossible as the release of information is limited by statute to that provided by or endorsed by official spokespersons. Two further statutes, the Protection of Information Act and the Laws on Cooperation and Development Act, both of 1982, extended this type of information control to more general areas of government activity. This type of legislation has a specific purpose: to distort the information system of the country in such a way as to deny factual data to anyone outside a closed circle, which excludes independent researchers. It is doubly insidious because lack of information inhibits the development of ideas (Mathews, 1980: 21-22) which might challenge those propagated by that group which has access to power and control of the media. The essential role of the researcher - to provide an independent and challenging role in society and promote its health by the infusion of ideas based on the collection and analysis of new, or perhaps forgotten, information - is fatally flawed. Statutory control of data has meant that the documentary record in our libraries on South African security methodology, penology, power resources, trade and sanctions and international relations is extremely scanty.

The sixth and final tier involves the banning of monographs, periodicals (other than registered newspapers) and various other forms of communication such as pamphlets and films. This is achieved via the Publications Act of 1974 and the Internal Security Act of 1982. The details have been documented elsewhere (De Jager, 1983; Merrett, 1985) but a summary is appropriate here. The Publications Act proscribes titles, some of which are also declared "possession prohibited". The Internal Security Act bans all the published work of banned and listed persons, who numbered 417 in August 1988; works published by or disseminating the policies of illegal organizations ("possession prohibited"); and certain titles classified as promoting the aims of communism (also 'possession prohibited'). A more detailed look at the censorship repercussions of the listing of persons is instructive. For example, all the work of the botanist Eddie Roux (which includes political economy and education) and the lawyer Albie Sachs remains technically banned. In the case of the political writer and journalist Ronald Segal his 1981 *State of the world atlas*, a standard work of reference of which he was co-compiler, is also banned.

Large libraries - those likely to be used by a majority of researchers - have blanket permits to hold and to lend to their members all that material which does not fall under "possession prohibited" categories, subject to certain provisos. These require that the titles in question are held under lock and key, a register of usage and a separate catalogue are kept, consultation takes place under supervision, photocopies are not made and only staff and postgraduates remove them from the library. In the case of the works of banned and listed persons access is not available to undergraduates, quoting is forbidden and the police have a statutory right to monitor the state of the collection. The mere possession of such literature is not an offence but circulation, importation, retail, display and continued production are forbidden, restricting acquisition and effective usage, in strict legal terms, to the larger libraries. The material deemed to be 'possession prohibited' in the three categories outlined above may not be held even by libraries without the express permission of the Director of Security Legislation or Directorate of Publications as relevant; nor may it be used by researchers without similar permission.

The censorship provisions of the Internal Security Act (which also influence the registration of new newspapers) are clearly political in nature. There is some confusion about the Publications Act, which controls the circulation of pornographic literature, but is clearly used as a political tool also. Much of the affected literature is concerned with political economy and political philosophy and in the period 1981/2 to 1986/7 92% of original submissions to the Control Board were made by agents of the State: customs and excise officers, the police and the Directorate of Publications itself. Only 8% were evidence of complaints or uncertainty by the public or publishers. From 1980/1 to 1986/7 8797 submissions were made of which 4324 (49.1%) were banned.[2] Over the past three years numbers of titles have been unbanned, mainly literary works, academic treatises on Marxism and dated socio-political analyses of the South African condition from the 1960s and early 1970s.

Notwithstanding the cautionary note sounded at the beginning of this paper it is intended to look in detail at two facets of censorship in an attempt to assess their impact upon research in the humanities and social sciences and in particular the availability of library resources. The two areas in question are the censorship provisions of the Internal Security and Publications Acts; and the State of Emergency.

Publications and Internal Security Acts

For academics and researchers the limitations placed upon the circulation and possession of monographs and serials are perhaps the most familiar form of censorship, probably because of its longevity. Numerous South African intellectuals have written on the problem although the literature tends to be fragmented and dated. It reached a peak in the late 1970s and has tended to fall away since then. There are a number of possible explanations: the emigration of those researchers who were the most vociferous opponents of censorship; the driving of others into lines of research which were unaffected by censorship; or the relative liberalization of the late 1970s in terms of library usage. Inevitably this sort of questioning has to remain speculative but it was decided to design a project to measure academics' perceptions of censorship in university libraries today.

In spite of the decline in intellectuals' criticism of the censorship system, powerful statements continue to be made about its effects. Brink (1984: 142) for example, sees censorship as an admission of the power of the written (and spoken and performed) word, a thought of considerable comfort to researchers and librarians. Boesak (1983: 55; 1984) records the fact that the South African authorities tried to inhibit Black theology through the censorship process. It is not likely that either the police or customs officials prize the truth as highly as researchers and intellectuals and as Cope (1982: 193) points out academic diligence is no guarantee against banning. Ten years ago Budlender (1979: 21) saw the existing censorship laws as placing the universities under a permanent state of emergency, a statement of considerable prescience given present circumstances. In general, recent writers have echoed the more frequently published sentiments of predecessors in blaming state censorship for encouraging academics to emigrate, indulge in the privatism of abstruse or non-contentious research, or succumb, wittingly or unwittingly, to self-censorship (Merrett, 1988: 185).

In mid-1987, 1352 academic staff at 7 departments in 20 South African universities were contacted by questionnaire.[3] There were 401 valid replies (or about 30%) from full time, permanent members of staff in departments of Fine Arts, History, Law, Philosophy, Politics, Psychology and Sociology. Their responses centred on four groups of questions as outlined below.

The first question asked whether university researchers had been hindered in their work by the inaccessibility of banned literature; and the specific research areas affected, the nature of the problem and the consequences. Of the respondents, 28% replied in the affirmative, constituting a significant minority. They derived from a variety of backgrounds but most worked at either the open or the black universities. Their replies showed that, in assessing the impact of censorship, traditional dividing lines between disciplines were not as significant as the research theme and the methodological approach. Thus the inter-disciplinary flavour of much modern research was demonstrated by academics from all seven disciplines working in at least one area from labour economics, black trade unionism, black politics and history; and suffering from censorship (table 1).

It was therefore not surprising to find that the main problem identified was access to theoretical work which ranked more highly than access to either data or research ideas (table 2). This order of priorities has about it a certain logic since for the alert researcher ideas are suggested by the socio-political context and data can be collected from sources other than documentation. Secondary sources are, however, the major origin of philosophical and political theory which helps to contextualize research. The consequences as shown in table 3 indicate that the major effect is seen as incomplete research which in some cases may be academically unsound as a result. With failure to start research and its premature termination ranked less highly it is clear that censorship subverts research more often than it prevents it altogether.

Secondly, respondents were asked if they had requested access to banned material in their university libraries in the last five years, and about the reaction of librarians. Table 4 indicates that 39% had done so including over half the respondents from History, Philosophy, Political Studies and Sociology, and a significant group from Fine Arts. The majority perception was that librarians are responsive to research needs rather than the law. A large group saw them adhering strictly to the law, although very few labelled librarians as super-censors. This picture is replicated in a supplementary survey conducted amongst university librarians. It is important to remember of course that these responses reflect perceptions only and it was not possible to gauge the depth of legal understanding of either academics or librarians.

The third question requested comments on the effect of State censorship and security legislation on research. A large number of respondents made no comment or misconstrued the question but of the 314 valid answers 65% indicated an adverse and 28% no effect. The replies of the majority echoed many of the sentiments of critics of censorship from preceding decades. Thus the impoverishment of research was mentioned, together with the fact that many areas are out of bounds altogether. A social psychologist noted the fact that he had given up a research field and retreated to the laboratory, a definite case of privatism. Self censorship was frequently mentioned as was the loss of research initiative and researchers to the less controlled environments of other countries. Some respondents felt that South African research was looked upon with suspicion because of access problems. Several recalled with nostalgia periods working among the collections of libraries in Europe and North America. Contrary viewpoints, however, held that inaccessibility of material encouraged a laudable search for less orthodox sources of data, while others thought that censorship could be evaded by various means.

Fourthly, the questionnaire asked how universities should respond to censorship. Excluding again the blanks and irrelevant comments (79) it was possible to group responses into broad categories. Only 6% of replies supported collaboration with the system and no fewer than 94% suggested some form of opposition. This spanned a wide range of ideas from civil disobedience to conciliatory approaches and negotiation. The most prominent single approach (30%) saw justification in special concessions for academics and thus supported the idea that freedom can be extended to the research community while denied to others, a line of reasoning which invites further debate beyond the confines of this paper.

State of Emergency

Many of the regulations embedded in the State of Emergency proclamations are primarily aimed at censorship, while others have implicit censorship effects. Taken together they represent a threat to academic endeavour of significant proportions. The regulations may be grouped into three main classes: those that impose prior censorship and allow for suspension of publications; those that restrain information gathering at source; and those that suppress anti-apartheid viewpoints. A fourth factor involves the extent to which the regulations exemplify the growing militarization of South African society, a development which has implications for resource building. The relevant regulations operative at the present stage of the on-going Emergency are contained in the Security and Media Emergency regulations together with the schedule of restricted organizations (South Africa, 1988).

Under the State of Emergency it is forbidden to publish information on security force action; restricted gatherings; certain strikes and boycotts deemed subversive; utterances of officials of restricted organizations; the circumstances, treatment and release of detainees; and advertisements published in respect of banned or restricted organizations. Nor may a publication print material which would have appeared in another had it not been for the suspension of the latter. Exemptions have been extended to government officials, most judicial proceedings (except those concerning detainees) - and bona fide libraries carrying out normal loan arrangements.

The latter exemption makes allowances for data published before the Emergency and perhaps for the small amount which has evaded the authorities since 1986. There is however a serious underlying implication for libraries contained in these regulations. The authorities are attempting to exclude from the published record essential facts about a broad segment of the current South African experience in the fields of political and economic unrest, police reaction, anti-apartheid politics and human rights. The implications for contemporary social scientists and future historians are clear, and have been spelt out concisely by a clinical psychologist:

> It is becoming increasingly difficult to publish the effects of detention on detainees because of legal limitations on the dissemination of information. A consequence of this is that there may soon be no adequate discussion of the clinical and ethical issues which necessarily confront the mental health professional who is treating detainees (Levin, 1988: 462).

A vast body of essential data will remain inaccessible to most researchers, locked up in the memories of individuals or in the vulnerable documentation of organizations which experience a precarious existence. Attempts to publish findings could well fall on the wrong side of the Emergency regulations and result, effectively, in the criminalization of intellectual activity.

The alternative Press has had the courage and motivation to challenge these regulations to the limit. They have found that creative reporting can be used to circumvent the prohibition on publication of the actions of the security forces - by recording instead, for example, the reaction of the crowd in such a way that the action is clearly understood. The *Weekly Mail* also argued that once a daily unrest report had been issued putting forward the State view, an individual newspaper could publish its own additional information and interpretation. This reasoning was rejected by the Rand Supreme Court in a case following seizure of the newspaper in August 1988. The potential for innovative reporting has gradually been narrowed by court decisions. In general South Africa's courts have devalued individual freedom, including academic freedom, in favour of the perceived needs of the State security mechanism.

Contained in the Emergency regulations are general powers of entry, search and seizure as well as specific authorization to seize publications and recordings. In September 1988 these powers were passed from the Minister of Home Affairs to the Minister of Law and Order. Such power has been exercised more energetically in the last year and it cannot be challenged in court. For example, in July 1988 14300 copies of the Learn and Teach booklet *The historic speech of Nelson Rohihlala Mandela at the Rivonia Trial* were confiscated even though excerpts had appeared elsewhere, including in a government publication; and in August an issue of the *Weekly Mail* was seized. More serious has been the case of *Al Qalam*, an anti-apartheid Muslim monthly, which last appeared in July 1988. It has been subjected to a continuous series of raids and the removal of documents and photographs, as well as the confiscation of the August edition. A fourth instance involved seizure of *Crisis News*, a Western Province Council of Churches publication, of which 30 000 copies were impounded in August. Such seizures must be viewed in the same light as the proscription placed on publication of sensitive information.

A third method of pre-publication censorship has been possible since August 1987 when new media regulations were gazetted. Since October 1987 the Minister has issued a steady stream of warnings to periodical publications and followed these to a conclusion in five cases by suspending them for periods of up to three months. This is the maximum suspension period for a registered periodical, although six months may be applied in the case of unregistered titles. The suspension process involves the right to make representations to the Minister, who has exercised his powers with considerable energy.[4] Up to 31 March 1989 twelve publications had been issued with first stage warnings and all except one represented anti-apartheid viewpoints. Two have ceased publication, one because its parent body was restricted in August 1988. Of the remaining ten, eight have been warned for a second or subsequent time. The purpose of this convoluted process, which has earned the South African government considerable international and domestic opprobrium, has been to curtial reporting of the aims, aspirations and activities of anti-apartheid organizations.

The *New Nation*, a Johannesburg weekly underwritten by the Roman Catholic Church, was closed on 22 March 1988 for thirteen weeks having challenged the Minister's powers in court. The Rand Supreme Court ruled against the paper arguing that doubtful material should be excluded. In the view of a sister newspaper, the *New Nation* was suspended because it recorded the violence of apartheid, the voice of the voteless and the aspirations of the oppressed (Weekly mail, 25 March 1988: 1). *South*, a Cape Town based Church supported weekly, was suspended for one month from 10 May, but both newspapers survived as they were independently funded and explored alternative methods of revenue raising. On 1 November the most important of the newspapers in the alternative tradition, *Weekly Mail*, was suspended for four weeks. Scrutiny of the material found objectionable by the Minister is instructive. He was disturbed by reporting of security force action, the activities and aims of banned and restricted organizations, and the views of conscientious objectors, all topics of crucial concern to researchers in South Africa today. The seriousness of the situation is emphasized by listing some of the offending articles: a feature on the constitutional guidelines put forward by the African National Congress which included analysis and some hostile comment; an academic analysis by a political scientist at University of the Witwatersrand on the state of the Pan Africanist Congress; and an article on the Secretary General of the South African Council of Churches.

The *Weekly Mail* in particular provides a high standard of writing and analysis on political, economic and human rights issues in South Africa. For example, it draws on the documentation of important and respected organizations such as the Human Rights Commission, Lawyers for Human Rights and the South African Council of Churches. *New Nation* and *South* report on news about the black townships and with a particular perspective that is not generally accessible to university and other researchers. Taken together, and with other threatened titles, they form an academic resource of growing importance in library collections, which fills the gap between the annual although increasingly belated publication of *Race relations survey*, a well known primary source of data on the South African condition. Of course these newspapers suffer from well known deficiencies. They are not academic publications and put together in haste and with commercial pressures they are vulnerable to bias, distortion and sometimes plain inaccuracy. Historians and other academic users of newspapers utilize them with care but enthusiasm. They are important because they are often the only documented source of data on current social and economic trends and they act as a channel for otherwise unrecorded viewpoints or attitudes. Without them, as the Minister it would appear would prefer us from time to time to be, an important research resource would be absent. It is possible that the suspensions so far experienced might be minor compared with future government action.

A second group of regulations restrains information gathering at source, an issue which goes beyond the control of published information to the matter of its original collection. This is of concern to researchers in two senses: the ability of third parties such as journalists to collect information and impressions; and their own ability to conduct fieldwork. In turn this determines the data which will be published in secondary sources such as newspapers, periodicals and monographs. There have of course long been restrictions on access to black areas of South Africa for journalism and research purposes, but the Emergency adds to these geographic limitations by topical restrictions. For example, reporters and photographers are required to remove themselves from the scene of security force action and the taking of photographs is forbidden. Any area, the scene of a strike, for example, may be declared an 'operational area', thus excluding observers. Researchers may thus be deprived of data in two ways: an inability to observe for themselves; or rely upon the observations of others who earn their living by recording events occurring around them.

The third type of censorship identifiable under the Emergency concerns direct action taken against individuals and organizations. For a number of reasons it is not known exactly how many people have been detained without trial since July 1985, but the figure exceeds 40000 (Human Rights Commission, 1988). Government secrecy, the Emergency regulations, harassment of human rights organizations and the sheer scale of events are contributory factors to this uncertainty. A main purpose of detention without trial is, however, crystal clear: to silence articulate anti-apartheid spokespersons and organizers. Their views and contributions to debate about the future of the country are securely locked up in prison in the same fashion as those of the many leaders convicted in political trials for the past three decades. Ex-detainees form the majority of those restricted under the Emergency.[5] Their restriction orders vary from person to person but generally they include a geographical limitation (to a given magisterial district) and a prohibition on publishing and public speaking as well as access to institutions such as newspaper offices involved in the dissemination of information. Such restrictions are now virtually routine on the release of high-profile detainees and they may, since 22 September 1988, be gazetted without any warning.

Parallel to the restriction of individuals has been that of organizations. On 24 February 1988 seventeen anti-apartheid organizations were fully and one partially restricted in such a way that they continued to exist legally but are unable to publish or propagate their own, or anyone else's, ideas. By the end of 1988 the number of restricted organizations had doubled. Their classification[6] shows that they were active in political, community, education, human rights, student and trade union issues. Thus research in the fields of black politics, community and youth organization, alternative education, trade unionism, conscientious objection and human rights (detentions, political trials, assassinations and disappearances) amongst others has been severely affected and another gap has been created in the documentation of South African affairs, although the South African Council of Churches and the Southern African Catholic Bishops Conference have taken up the role of some of these organizations and have their own documentation procedures and skills. Some restricted organizations have appeared under new names and others, such as the Human Rights Commission, have emerged in response to the new situation. By the end of 1988 there were signs that the disrupted documentation process was being re-established, especially under the protection of appropriate university departments and institutes. Some saw in a more active documentation role, formerly left to others, an appropriate academic response.

Mention has been made of the growing militarization of South African society, a process which has been underway since the late 1970s. Censorship, which used to be the preserve of government bureaucrats is now shared with the growing number of apparatchiks wearing uniform, and there is good reason to believe, looking at South Africa's history of censorship, that their potential power in this area is virtually limitless. At the local level the National Security Management System (NSMS) is represented by Joint Management Centres (JMCs). One of their four main areas of concern is communications based on the belief that South Africa is experiencing a low intensity civil war in which words and thoughts are weapons. The effect of this development on documentation remains to be seen but its potential is obvious.

The Emergency regulations may thus be seen as a microcosm of South African censorship as a whole: severing channels of communication between different sectors of society; silencing dissident individuals and organizations; shielding government policy and methodology; and finally banning those publications which try to circumvent the system. The result is disastrous for general discourse and a more complete understanding of South African society, without which it is more difficult to plan ways in which it might be changed. In short it is a catastrophe for a wide variety of types of research and shows how the continued existence of the South African status quo depends upon intellectual repression. This suggests that although there are countless historical, social and political differences between South Africa and Eastern Europe there might be value in international comparisons. In the case of Czechoslovakia Milan Kundera (1983: 235) writes of "organized forgetting", in other words a State orchestrated attempt to erase from the historical record, and presumably from the popular consciousness, significant areas of fact and ideas.

Two prominent anti-apartheid academics echo this by arguing that " ... our struggle is also a struggle of memory against forgetting" (Suttner and Cronin, 1986: 4). Libraries are being progressively cut off from wide areas of South African life and their resources are becoming a monument to our ignorance rather than our understanding of the national condition. It is possible that these documentary lacunae will never be filled even under a different political dispensation. Some material is of course available in overseas collections and there is a new trend to expatriate, for reasons of security, vulnerable documentation centres,[7] but these are not always easily accessible for South African researchers. At the same time there is the irony under a State of Emergency, which was ostensibly declared to preserve law and order, of the increasing vulnerability of small library and resource centre collections and the documentation of anti-apartheid organizations to arson attacks and other acts of violence by persons whom the authorities have been unable to identify.[8]

Conclusions

It seems appropriate in the light of the above to consider three issues in conclusion. Firstly, there is, I believe, virtue in disaggregating censorship and comparing its elements. The two considered in this paper are particularly apposite. The banning of books and other material under the Publications and Internal Security Acts can be described crudely as "traditional" censorship which has been in place now for 25 years, since the passing of the Publications and Entertainments Act of 1963, the first statute systematically to censor material published in South Africa. There is evidence, quantitative and qualitative, to show that it has been and remains a problem for research and researchers in South Africa, but one which the innovative and the imaginative have sometimes been able to circumvent. This has been possible because of a number of internal contradictions in the bureaucratic methods required to sustain it: the need to use librarians, who are not always cooperative, to police the system; the difficulty of documenting all the published work of banned and listed persons; and the impossibility in a relatively open society with strong links with other countries of controlling the world's literature.

On the other hand researchers have since 1985/1986 been faced with the censorship implications of States of Emergency which can be categorized as "radical". Taken together they symbolize most of the different types of censorship which have plagued South Africa. The State is correct to call its Emergency a "total response", a medley of repressive tactics and suppression of human rights which has, it is fair to argue, severe implications for future research. Some of these implications have yet to reveal their full potential.

Consideration of these two facets of censorship gives an indication of the shifting balance within government strategy. The survey revealed the fact that "traditional" censorship obstructed the acquisition of contextual theory rather more than the collection of data or research ideas. The Emergency it might be argued has little effect on theoretical work and might indeed stimulate thought about lines of research, so dramatic are the events unfolding around us. It is beyond doubt, however, that the Emergency has had a devastating effect on the collection of the raw material of the socio-political and economic condition of South Africa and this is destined to have radical consequences for the prosecution of research in many fields. We are at present experiencing a crucial shift in the tactics surrounding State interference in the intellectual process.

Secondly, given the historical and contemporary state of censorship in South Africa, it is justifiable to characterize it as a censored society. Intellectual activity was placed under a State of Emergency well before 1985 and some of it has been potentially criminalized. The violence of censorship has been directed at areas which belong exclusively to the humanities and social sciences: there is no instance on record of anyone in the pure or technological sciences suffering such obstruction for research per se, however radical in conception. Lack of accessible documentary material concerning a wide range of key topics has had a stifling effect. Most research in the humanities requires as reference point or contextual framework a background of documentation from which to strike out in new directions. For example, a recent instance to come to my notice was that of a researcher investigating the issue of capital punishment in South Africa who has discovered that only the barest skeleton of published material exists upon which to base new research. There is enough evidence to conclude that privatism has been a major problem in South Africa; that contentious topics which will find no favour with or even harassment from the authorities have been avoided; that much research has been abandoned or left incomplete; and that much more has been exiled.

The third point is more speculative and ends this paper on a suitably questioning note. Given the policy of the State towards intellectual endeavour in so many fields what should be the reaction of the research community? It is possible to argue that self censorship, conscious or subconscious, now plays a significant role in research in the humanities in South Africa, although there would no doubt be much disagreement about the extent to which this takes place. Opponents and supporters of government policy periodically raise the issue of academic freedom in public debate. The history and impact of censorship are such that there is a good case for promoting the issue of academic responsibility and asking two questions: what should researchers be doing to combat State censorship; and how does this tie in with their duties and obligations to society as a whole?

Footnotes

1. The figures per child for 1986/7 (including capital expenditure) are as follows: White, R2508; Indian, R1904; Coloured, R1021; African in White areas, R477; Transkei, R413; KwaZulu, R359; Gazankulu, R328; KaNgwane, R280; figures for the other bantustans being unavailable. Source: RACE relations survey, 1987/8. Johannesburg: South African Institute of Race Relations, 1988, 151.

2. The detailed figures are:

Police	5 312	58.5%
Customs	2 369	26.1%
Directorate	671	7.4%
State agents	**8 352**	**92.0%**
Publishers	433	4.8%
Public	297	3.2%
Private sector	**730**	**8.0%**
TOTALS	**9 082**	**100.0%**

Sources: SOUTH AFRICA. Department of Internal Affairs *and* Department of Home Affairs. Annual reports, 1980/1-1983/4; 1984/5 and 1986/7.

3. This survey was financed by the Human Sciences Research Council and, entitled "State censorship and the academic process", formed part of the HSRC's Investigation into research methodology.

4. The Minister's actions, up to 31 March 1989, can be summarized as follows:

Title	**1st warning**	**2nd warning**	**Outcome**
New nation	Oct 1987	Dec 1987	Suspended 22 Mar-10 Jun 1988
Work in progress	Nov 1987	Dec 1988*	
Die stem	Nov 1987	Dec 1987	Ceased Dec 1987
Sowetan	Nov 1987		
South	Nov 1987	Dec 1987	Suspended 10 May-10 Jun 1988
Weekly mail	Dec 1987	Apr 1988*	Suspended 1 Nov-28 Nov 1988
Grassroots	Apr 1988	Jan 1989	Suspended 17 Feb-16 May 1989
Out of step	Apr 1988		Parent body restricted Aug 1988
Saamstaan	Apr 1988		
New era	Jun 1988	Jan 1989	Suspended 17 Feb-16 May 1989
Free Azania	Nov 1988		
Al Qalam	Jan 1989	Feb 1989	

* and third or subsequent warnings.

5. There are currently about 800 persons restricted under the State of Emergency most of whom may not be quoted. Source: *Southscan,* 4(14) 1989, 108.

6. The 32 organizations restricted during 1988 may be classified as follows:

Community (5)
Cradock Residents Association (CRADORA) 24 Feb 1988
Port Elizabeth Black Civic Organization (PEBCO) 24 Feb 1988
Soweto Civic Association (SCA) 24 Feb 1988
Vaal Civic Association (VCA) 24 Feb 1988
Western Cape Civic Association (WCA) 24 Feb 1988

Education (4)
Democratic Teachers Union (DETU) 29 Dec 1988
National Education Crisis Committee (NECC) 24 Feb 1988
National Education Union of South Africa (NEUSA) 24 Feb 1988
Western Cape Teachers Union (WECTU) 29 Dec 1988

Human rights (5)
Detainees Parents' Support Committee (DPSC) 24 Feb 1988
Detainees Support Committee (DESCOM) 24 Feb 1988
End Conscription Campaign 24 Aug 1988
National Detainees Forum (NDF) 29 Dec 1988
Release Mandela Campaign (RMC) 24 Feb 1988

Political (5)
Azanian Coordinating Committee (AZACCO) 26 Oct 1988
Azanian Peoples Organization (AZAPO) 24 Feb 1988
Blanke Bevrydings Beweging van Suid Africa (BBB) 17 Nov 1988
Committee for the Defence of Democracy 12 Mar 1988
United Democratic Front (UDF) 24 Feb 1988

Trade unions (1)
Congress of South African Trade Unions (COSATU) partial restriction 24 Feb 1988

Youth (13)
Azanian Youth Organization (AZAYO) 24 Feb 1988
Black Students Movement (BSM), Rhodes University 8 Dec 1988

Black Students Society (BSS), Wits University 8 Dec 1988
Cape Youth Congress (CAYO) 24 Feb 1988
Mitchells Plain Student Congress (MSC) 13 Dec 1988
Port Elizabeth Youth Congress (PEYCO) 31 Oct 1988
South African National Students Congress (SANSCO) 24 Feb 1988
South African Youth Congress (SAYCO) 24 Feb 1988
Soweto Students Congress (SOSCO) 26 Oct 1988
Soweto Youth Congress (SOYCO) 24 Feb 1988
Transvaal Students Congress (TRASCO) 31 Oct 1988
Western Cape Students Congress (WESCO) 29 Dec 1988
Western Cape Students Council (WSCS) 13 Dec 1988

7. For example, in November 1987 the South African Council of Churches decided to exile its Ecumenical Documentation and Information Centre for Eastern and Southern Africa (EDICESA) to Harare, Zimbabwe in view of the fact that one of its aims is the documentation of the South African regional policy of destabilization.

8. On 13 April 1986 the Resource Centre of the Durban Ecumenical Centre was damaged by a petrol bomb. In May 1987 COSATU House in Johannesburg was irreparably damaged by a massive explosion; and during 1988 the Community Resources and Information Centre (CRIC) and Khotso House in Johannesburg; the Black Sash Office in Port Elizabeth; and Khanya House in Pretoria were all damaged to a greater or lesser extent by fire and/or explosions. The effect of such attacks on documentary resources is being investigated.

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Weekly Mail 25 March 1988, 1.

TABLE 1

Has your research work ever been hindered by the inaccessibility of banned literature?
If YES, please describe the particular areas of research affected

RESPONSE	A	B	C	D	E	F	G	TOTAL
YES	8	21	24	9	21	17	11	111
NO	17	41	92	16	13	83	28	290
%YES	32.0	33.9	20.7	36.0	61.8	17.0	28.2	27.7

AREAS OF RESEARCH AFFECTED

African National Congress	B, E
Alienation	A
Angola	E
Apartheid, psychology of	F
Art, erotic	A
Art, political (South Africa & Third World)	A
Art, pop	A
Banned organizations	C
Civil liberties and human rights	C
Collective behaviour	G
Conflict handling & crisis management	F
Consciousness	F
Culture, black & ANC	A
Decolonization	F
Detention & solitary confinement	F
Education, black	B
Eroticism	A
Fanon	E
Feminist studies	A
Government decision making	C
Group areas	B
History (South Africa & Africa)	B, F, G
History, urban (South Africa)	B
Judicial processes	C

Justice, ANC perspectives	C
Labour economics, control and relations	A, B, F, G
Law, constitutional	C
Law, customary	C
Law, labour	C
Law, people's	C
Law, politics of	C
Law, private	C
Law, security	C
Law, sociology of	C
Legal system (South Africa)	C
Legal systems, socialist & Marxist	C
Literary theory	G
Marriages, mixed	C
Marxist theory	A, E
Mental health	F
Mozambique	E
Namibia	C, E
Nationalism (South Africa & Africa)	B
Passive resistance	B
Political economy	G
Political opinion	C
Political theory	B
Politics, black South African	B, E, F, G
Politics, extra Parliamentary	G
Politics, opposition South African	B, E
Politics, racial	C
Politics, revolutionary	E
Pornography	A
Prisoners, political	C
Prisons	C
Psychology, African	F
Psychology, colonial	F
Psychopolitics	F
Racial prejudice, social psychology of	F
Resettlement	A
Resistance movements	G
Restricted persons	C
Revolution, theory of	B
Social theory	G
Socialism	E
Sociology, military	E

South African Communist Party	B, E
Soviet Union	E
State, theory of	G
Torture	E
Trade unions and black workers	C, D, F, G
Transkei	G
Warfare, guerilla	E

Abbreviations

A = Fine Arts
B = History
C = Law
D = Philosophy
E = Political Studies
F = Psychology
G = Sociology

Source: State censorship and the academic process: A survey undertaken as part of the HSRC Investigation into Research Methodology.

TABLE 2

Has the problem been:
inability to frame basic research questions (1)
lack of access to relevant theoretical work (2)
denial of factual information (3)
other factors (4)

RESPONSE NO.	A	B	C	D	E	F	G	TOTAL
HINDERED	8	21	24	9	21	17	11	111
% AFFECTED BY								
1	00.0	19.0	20.8	22.2	14.3	29.4	18.2	18.9
2	87.5	47.6	83.3	55.5	66.7	82.4	81.8	71.2
3	75.0	61.9	70.8	22.2	61.9	52.9	81.8	62.2
4[a]	12.5	14.3	00.2	33.3	04.8	00.0	00.0	08.1

Note

a) Factors identified were:

Delays	B, C
Inability to quote	D
Isolation	D
Special permission required	D
Use of secondary sources	B
Unspecified	E

A related factor noted was:

Restriction on township fieldwork	A

Abbreviations

A = Fine Arts
B = History
C = Law
D = Philosophy
E = Political Studies
F = Psychology
G = Sociology

Source: State censorship and the academic process: A survey undertaken as part of the HSRC Investigation into Research Methodology

TABLE 3

Have the consequences been:
failure to start research (1)
premature termination of research (2)
incomplete or academically unacceptable research (3)
other factors (4)

RESPONSE NO.	A	B	C	D	E	F	G	TOTAL
HINDERED	8	21	24	9	21	17	11	111
% AFFECTED BY								
1	37.5	04.8	20.8	33.3	28.6	29.4	27.3	23.4
2	25.0	04.8	08.3	00.0	19.0	11.8	9.1	10.8
3	25.0	66.7	75.0	44.4	61.9	64.7	54.6	59.5
4[a]	37.5	14.3	12.5	44.4	23.8	23.5	27.3	22.5

Note

a) Factors identified were:

Delays	B, C, D, F, G
Use of material abroad	A, C, E, F
General anxiety about the law	C, D, E
Lack of motivation, frustration and shift to other research	D, F, G
Consequential banning of work	A
Resort to field work	A

Abbreviations

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B = History
C = Law
D = Philosophy
E = Political Studies
F = Psychology
G = Sociology

Source: State censorship and the academic process : A survey undertaken as part of the HSRC Investigation into Research Methodology

TABLE 4

Have you tried to obtain banned material from your university library within the last five years?

RESPONSE	A	B	C	D	E	F	G	TOTAL
YES	9	36	29	15	26	17	22	154
NO	15	26	87	10	8	83	17	246
%YES	37.5	58.1	25.0	60.0	76.5	17.0	56.4	38.5

One respondent failed to answer the question

If YES, have the reactions of librarians, in your opinion, been one of the following?:

responsive to the needs of research rather than the demands of the law (1)
strict adherence to the law (2)
more restrictive than the law requires (if so please specify) (3)

	A	B	C	D	E	F	G	TOTAL
PERCENTAGE REACTION								
1	66.6	50.0	32.0	60.0	65.4	58.8	43.2[a]	51.3
2	22.2	47.2	67.9	40.0	30.8	41.2	56.8[a]	46.7
3[b]	11.1	02.8	00.0	00.0	03.8	00.0	00.0	02.0

a) One respondent chose a mix of 1 and 2 and this answer has been divided pro rata

b) Examples quoted were the restriction of a book on the basis of its dustjacket; and the retention of unbanned material in the banned book cupboard

c) Miscellaneous responses included:

Building of personal libraries	F
Use of a nearby national library to avoid university library restrictions	B

Abbreviations

A = Fine Arts
B = History
C = Law
D = Philosophy
E = Political Studies
F = Psychology
G = Sociology

Source: State censorship and the academic process: A survey undertaken as part of the HSRC Investigation into Research Methodology.

14

Research Ethics in the Social Sciences

Eleanor Preston-Whyte

The topic of ethics and ethical standards in social research is a contentious one. Some social scientists will brook no interference in their research and publication, while others grudgingly acknowledge that some controls may be necessary. Those who have experienced the repercussions of what they believe to be unethical behaviour in both fieldwork and publication, often call for strong measures to prevent similar occurrences in the future. As a result of this type of pressure most disciplines have, at one time or another, introduced formal or informal controls, through either the formulation of a Code of Ethics[1] which binds the members of a professional association, or through the compilation of a list of ethical guidelines which provide a generally agreed upon basis for making informed decisions about both fieldwork and publication. Dissenting voices are, however, invariably heard and differences of interpretation often occur in relation to particular cases and are particularly characteristic of highly polarized social and political situations. Equally, and possibly more divisive, are the controversies over ethical issues which occur as a result of changes in the view which researchers, or some of the members of a discipline, have of their role in society and of the proper relationship which researchers should have with the communities they study. In these cases the controls and sentiments expressed in both codes of ethics and ethical guidelines may soon be rendered out of date and inappropriate (Ellen, 1984: 134-135). It may, indeed, be that we are experiencing just such a period of change in the social sciences in contemporary Southern Africa.

The objective of this paper is not only to raise the general issue of research ethics in the social sciences in this country and at this time, but, more specifically, to consider the appropriateness of attempting to formulate guidelines for professional conduct, either within particular disciplines, or (and this would be a far more daunting proposition) for the social sciences as a whole. The Research Code which guides work done within the HSRC is one example of

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211

an attempt to formulate both a basic philosophy of social science research and a set of standards applicable to the wide range of disciplines within which research and publication is undertaken by this body. More recently, one of the two associations of professional anthropologists in the country has accepted a less restrictive, but rather more extensive document, aimed at drawing attention to the wide range of dilemmas which may arise in the course of anthropological fieldwork and publication. Acceptance of these guidelines is a condition of membership of the newly constituted Association for Anthropology in Southern Africa (hereafter referred to by the acronym AASA). Although agreed to by most potential members, it has been claimed that certain of the guidelines have kept some South African anthropologists from joining the Association. The accusation levelled against the Association is that the clauses at issue make a political statement and seek to regulate and exclude professional colleagues on grounds which should be a matter of personal conscience and not professional ethics. Controversies such as these point to radically divergent views of what the role of anthropology in contemporary southern Africa can and should be. This and the nature of the power relations between researcher and researched are,indeed, themes which will dominate the discussion which follows.

Anthropologists are by no means alone in their concern over the issues outlined above. Multidisciplinary research is on the increase in this country and many of the research techniques of classical anthropology are being adopted both by other social scientists and also by researchers and applied scientists trained in fields as diverse as medicine, agriculture and engineering. There is a growing awareness of the need to "take people into account" and for the micro-level research which will provide the focus necessary for this. The qualitative techniques of in-depth interviewing,the collection of detailed case histories and participant observation (Ellen, 1984: 138-139,143) are no longer the preserve of anthropology and as other social scientists adopt these intensive and personal fieldwork methods, so they experience the problems and both professional and personal dilemmas which have beset anthropologists for decades. Of these the most crucial is that of how the interests of informants or (to use the term now preferred in many circles research participants,[2] may best be protected.

Actions which have resulted in widespread opprobrium within the anthropological community range from involvement with the military (Jones, 1971; Lee and Hurlich, 1982; Gordon, 1988), secret research and taking funds from interested bodies which it is known will seek to monitor the publication of the results or control either the course of the research or the use to which the results are put (Barnes, 1978; Ellen,[3] 1984). These issues, are, however,often far from clearcut. In war (see,for instance Beals 1969: 55) or if researchers feel that their work may aid the fight against injustice and tyranny (van den Berghe 1969), not only may both they and their colleagues feel justified, but both may accept a moral and professional obligation to put research expertise at the disposal of those they consider to be in the right. A grey area exists, of course, in deciding exactly who is in the right or even what constitutes right, and it is here that social scientists may experience difficulty not only in choosing between alternatives, but in balancing their opinions and responsibilities as scientists, as citizens and as private persons (Ellen, 1984: 133,143-144,143). In the case of research which is planned to have a direct bearing on the future of the people concerned, assessments of what is likely to be beneficial and what will be harmful may be the center of dispute, as may the motives of the research funders, witness the controversy over accepting research money and contracts from state bodies and even from para-statals such as the HSRC (Cloete, Muller and Orkin, 1986).

There is one score on which all anthropologists agree. The research process should not be allowed to result in any direct harm, either physical or mental to informants and the researcher has the responsibility to anticipate and prevent any such occurrence. Again, however, the definition of what constitutes harm may differ, both between researchers at any one point in time and, more particularly, as the discipline has changed in the face of theoretical or political critiques (Ellen, 1984: 135-137). In situations characterized by an unequal distribution of wealth and power it often happens, moreover, that the researched come largely from the category of those who have little formal control over their lives. They are also usually drawn from the "have nots" rather than the "haves"of this world (Jorgensen, 1971). In contrast, researchers tend to come from amongst the wealthy and educated and may also be recruited predominantly from the ranks of those in power. Even if this is not the case, most researchers have at least managed to gain the support of powerful gatekeepers and decision makers. These factors may make them insensitive to nuances of oppression which are felt strongly by those whom they study. It may also mean that researcher and researched have very different perceptions of what constitutes harm, both at the time of research, and in the longer term.

Harm to research participants has two dimensions. It is important to distinguish harm in the sense of repercussions for the community at large and harm which might personally befall particular informants because of information which the researcher has collected in the course of fieldwork. It is the second contingency which is not always fully appreciated by fieldworkers and which, the closer the researcher works with his or her research participants, becomes increasingly likely.

1. Confidentiality and protecting the anonymity of research participants

Much anthropological fieldwork involves an extended period of living in and with the community being studied, participating in daily routines and getting to know many of the people intimately, indeed converting some of the latter from mere informants into friends. Even if other social scientists do not go to quite these lengths, many are spending considerable periods of time in participant observation, and this necessarily exposes the investigator to a wide spectrum of social and community activities. They often become party to occurrences and discussions which are not directly relevant to their current research interests and which, moreover, the people concerned, even if they are generally well disposed to the aims and objectives of social research, would not normally wish to be revealed to outsiders. It is here that serious thought needs to be given to what may be reported, what should remain confidential, to the legitimate and illegitimate use of certain types of information gained in the field and, above all, to preserving the confidentiality of research material and the anonymity of informants and of the communities to which they belong. These issues recently became the center of both academic and public debate following the publication of a book by Vincent Crapanzano entitled *Waiting: The Whites of South Africa* (1985).

Crapenzano, an American anthropologist,came to this country in order to study white South Africans. He established himself in a small Cape village which he later referred to in his book as Wyndal. The descriptions which he gave both of the physical setting of the village and of some of the villagers themselves allowed both to be easily identified. Using case studies and what amounted to verbatim quotations from both interviews and informal discussions, he painted what to the villagers seemed a largely negative picture both of the village as a whole and of particular people. Not surprisingly many of his informants were distressed and angry especially when reports of his work hit the Sunday press (Sunday Tribune: August, 18 1985). While they and many of the academics who later commented on the case (Boonzaier, *et al.* 1985; Hugo, 1985) acknowledged Crapanzano's right to report negatively on what he had found in his fieldwork, what stuck in most throats was the fact that, by not ensuring the anonymity of particular people, he had caused them considerable embarrassment and hurt. For their part, the villagers complained that he had abused their hospitality and in some cases broken confidence with them in reporting what they had thought would be treated as "off the record". One person pointed out that although she had known that Crapanzano was collecting information for a book - for he did not hide this fact - and she had been happy to grant him an interview and to answer the questions he put to her in this formal situation, she had not expected what she had said informally over dinner to be reported as well. In this sense the villagers felt that they had been exploited and their hospitality abused and it is difficult to disagree with them.[4]

Cases such as the above[5] reflect badly on social science research and the situation would have been much worse had the material been of such a nature as to result in permanent harm to those concerned, persecution by neighbours or even prosecution by the authorities. In many of the situations in which researchers find themselves in this country such eventualities seems only too frighteningly possible and the point hardly needs to be made that it is not merely published material which might constitute a threat to people's safety, but fieldnotes and even unwritten knowledge absorbed in the course of investigating something quite different. In a highly polarized political situation such as this it is difficult to avoid such complications. In South Africa, as elsewhere in the world, neither social scientists nor their research material are protected or privileged under law and nor are researchers immune to pressure from powerful agencies such as the police and courts (Ellen, 1984: 144). This point is made in the Ethical Guidelines recently adopted by AASA where anthropologists are warned that their research data may be subject to legal subpoena (clause 1(ii)). If indeed it seems clear to researchers that they may become party to information that could be detrimental to their informants they are encouraged to abort the research even if this involves them in personal and professional loss. Such a decision would be a difficult and bitter one to take, but the following paragraph from the preamble to the Guidelines, not only sets out some of the dimensions of the dilemma, but, by voicing corporate professional thinking on the matter, helps to give the courage necessary for such drastic action.

> The close and often lengthy association which anthropologists have with the people amongst whom they carry out their research gives rise to moral and personal relationships, to mutual trust and reciprocity. This and the fact that anthropologists often work in situations characterized by differential access to power and resources imposes upon them a grave responsibility to consider carefully the nature of their research and its likely results for their research participants. If it seems at all possible that the latter will be affected detrimentally or will consider themselves to have been harmed by the study, the researcher should not embark on the research or should abort it if it is already in progress (Preamble: AASA 1988).

The potentially harmful nature of much social research raises the issue of secrecy, both in research itself, and in the use to which research material is put and it is to these controversial matters that we must now turn.

2. Secret or confidential research and the use of covert research methods

Controversy invariably erupts in anthropological circles over instances of secret or confidential research and also the use of covert research methods in the field. It is most heated in cases in which the research is being conducted by or on behalf of governments or the military for there is always suspicion in these cases that the work has been designed for and will be used to the detriment of the people concerned. Similar objections are raised to confidential consumer research undertaken on behalf of powerful companies and to any covert work aimed at promoting production, be it either in the workplace directly, or in connection with community or welfare services provided by such companies.

The general argument against covert research is that individuals have the right to refuse to give information or to have their lives and actions scrutinized by others. If this makes the study impossible, this must simply be accepted and no recourse to arguments phrased in terms of the greater good of the majority or the inalienable value of scientific knowledge are acknowledged. By and large this is the stance in anthropology and arguments such as that made by van den Berghe (1971) who justified misrepresenting the nature of his research both to the South African authorities and to his informants on the basis of his moral objections to the apartheid regime, are highly controversial. Representing oneself (and ones professional self at that) as something which one is not is regarded by most anthropologists as tantamount to spying. The reason why this hard line is taken is because anthropological fieldwork might easily and (it is claimed) has been used as a cover for espionage and counterespionage (Boas, 1919, reprinted in Weaver, 1973: 51-52). In fact the typical participant observer is doing no more nor less than what a successful spy might attempt - witness the actions of Crapanzano (Ellen, 1984: 145-146). The anthropological attitude to covert research is part of the belief that the research participant is an equal partner in research and is dealt with in the AASA Guidelines under the heading of "Avoiding undue intrusion" where we read:

> Anthropologists should be aware of the intrusive potential of some of their enquiries and methods. Researchers have no special entitlement to study all phenomena, and the advancement of knowledge and the pursuit of information are not in themselves sufficient justification for overriding other social and cultural values.

Turning to the issue of secret or confidential research, that is to research the results of which are not made generally available, the position was once almost as rigid. In the Principals of Professional Responsibility of the large and prestigious American Anthropological Association it is stated quite categorically that "in accordance with the association's general position on clandestine and secret research, no reports should be provided to sponsors that are not also available to the general public and, where practicable, to the population studied" (American Anthropological Association 1971: paragraph 1g). In another clause the same Code recommends that its members refuse research opportunities if it seems likely that the results of this work "cannot be freely derived and publicly reported" (ibid: paragraph 3a). The arguments for this position turn not only on the presumption that what is kept secret is unlikely to be to the benefit of those concerned but, also, on the fact that unpublished work is not open to peer review and to the control of standards in both fieldwork and in the treatment of informants that publication and general openness usually ensures.

It should be explained that the American Anthropological Association's Principles of Professional Responsibility were drawn up in the late nineteen-sixties when, in the aftermath of the Project Camelot[6] debacle (Sjoberg, 1969; Horowitz, 1973), with its connotations of counterinsurgency research, it seemed imperative to take a firm line on the issue of free and open publication. As time has elapsed, however, it has become clear that matters are not as clear cut as they seemed at that time. Today sponsorship comes from a wide range of sources and consideration has to be given to market research and to work of an applied nature which sponsors wish to keep confidential from competitors. If anthropology is not to remain merely an academic pursuit, thus limiting drastically the numbers who can join the profession, a more realistic approach this topic is, perhaps, called for. It is also possible that the request for confidentiality may come, not from a powerful sponsor, but from the community in which the work is being done. If, as is argued below, members of communities have the right to become involved in the research process, they have surely the right to ask that the findings of the research be kept private. In an increasing number of cases it may be communities themselves which sponsor research or find the financial backing for it. They may take the position that the results are their business and do not concern outsiders. They may also deny the right of researchers to use the results of their research experiences for general academic or scientific purposes. The decision which then faces the researcher is whether it is professionally worthwhile or, some would ask, even justifiable to undertake the research. On the other hand there is a significant lobby which argues that social scientists have a moral responsibility to answer such calls on their expertise and that an insistence on full publication is merely part of the selfish career orientation of many ivory tower academics.

There is yet another possibility to be considered. It may happen that researchers themselves see the need to desist from publication. The reasons may be related to the impossibility of ensuring the anonymity of research participants such as public figures or to the fact that, as the research continues, researchers come to suspect the motives of sponsors which at first they believed to be unproblematic. In other cases political situations change and the material they are collecting, or merely the region in which they are working, may become sensitive. Many researchers doing fieldwork in areas in which new mineral resources are discovered, which become international flash points or become the subject of national and international population moves and social engineering, have experienced what it is like to have their work suddenly become the object of intense non-anthropological interest. Indeed any information published on these areas, however innocuous it seems to the writer, may be of use to the authorities and in this respect many social scientists have proved to be extremely naive. A specific instance of this nature is examined by Jones (1971) and the possible parallels with the situation in this country and, indeed, with the southern African region as a whole, cannot be escaped (Lee and Hurlich, 1982). Ackeroyd sums up the situation admirably when she says in a chapter on ethics in a recent reader on anthropological fieldwork (Ellen, 1984: 141) that certainty and a hard line on the topic of publication "begin to seem academic and unrealistic and, it might be argued (in some political and research contexts), themselves unethical".

This perspective has been made itself much felt in those situations where anthropologists have entered the fields of development, action and advocacy research (Foster *et al.* 1979)[7] and find themselves confronted by more than usually problematic ethical issues. These may center on the negotiation of power relations between government and the communities in and with which they are working. Confidentially may not be possible or advantageous in the usual sense, and the community may not wish its problems and experiences in achieving its development goals to be publicly chronicled. They may find it as offensive as did the villagers of Wyndal to read about the factions in their community and it seems likely that their leaders will be as easily identifiable as were the main figures in Crapanzano's book. Some people are sensitive about the very exposure of their poverty and others would inevitably be portrayed in a less positive light than they see themselves. There are clearly some confrontations, both with outside authorities, sponsors and gatekeepers of various kinds and with representatives of internal factional struggles which it would not be in the interests of either the community or of particular individuals to reveal. Without these illustrations, however, many of the insights into the development process would lack their force. This is, of course, a problem facing all social science writing which is based on contemporary field material, but in the case of development planning, an incautious word may easily alienate an important sponsor or it may ruin the career of a political leader. Taken all in all it may seem ethical not to publish rather than to put the objectives of the research and some of the research participants in jeopardy.

A closely related area on which there is currently a good deal of soul searching is the proper arena for the publication of both pure and applied research results. It is all very well for academic papers to result from such work, but the question is raised of the extent that these publications are accessible or even of interest to the communities in which the work was done. In response to this type of criticism some authors write more for community based papers than for academic journals or they seek to replicate their material in a modified form appropriate to different audiences.

When the guidelines for the AASA were being drawn up it was clear that a statement on the issue of confidential research and of the position of the Association on the non publication of certain material had to be made. After much discussion it was decided that although, on balance, the members were against secrecy in research reporting and in the provision of secret reports to sponsors, they were also aware of the strength in some situations, and particularly in tense political or development contexts, of contradictory ethical imperatives. The need in some cases to withhold material from publication was appreciated as was the point of view of sponsors wishing to protect themselves from competitors. As a compromise in the latter case it was suggested that researchers doing contract research should negotiate a period of embargo with their sponsors after which the material could be published freely. This suggestion was included in the AASA Guidelines in which the whole topic was dealt with in some detail as follows:

> Many anthropologists are against the practice of secret research involving both the use of covert research methods and the submission of confidential reports to sponsors. Anthropologists, may however, be employed by agencies which demand confidentiality in reporting some research results. Where secrecy is requested by sponsors or employers, anthropologists should consider the issue in the light of all the points in this document. They should negotiate with their employers or sponsors for the right to publish freely in academic journals even if this takes place only after a period of embargo. In some cases it may be the anthropologist who finds that confidentiality in reporting is necessary to protect research participants (AASA Guidelines 1988 6(i)).

A similar strategy to that of a temporary embargo on the publication of material commissioned by sponsors could, of course, be used by all social scientists in cases of material which they feel is personally or politically too sensitive to be made public at the time of research. The period of such an embargo would have, in all probability, to be in terms of decades or even quarters of a century in order to avoid a "time bomb"effect (Ellen, 1984: 148).

3. Confidentiality in fieldnotes and in the use of data storage systems

Publication is only one aspect of the confidentiality problem. It is quite possible that uncontrolled access to fieldnotes and to copies of interview schedules could be as, if not more, damaging to the interests of research participants as the published results (Ellen, 1984: 147-149). In the latter the author might well protect the anonymity of informants, perhaps through the use of effective pseudonyms, but anyone else using that material might be less scrupulous. Clearly raw material needs to be checked and coded as carefully as does published results and suggestions that field notes and copies of other field data should be placed in public depositories requires serious and critical consideration, and, perhaps the imposition of an embargo, both in terms of the time before it can be generally used, and in terms of who may have access to it, and for what purposes. Similar problems and solutions should be anticipated and worked out in the case of collaborative research with members of ones own discipline and more especially with researchers from those other disciplines in which, perhaps, the issue of confidentiality is not felt strongly. These problems are dealt with in the AASA Guidelines under clauses 6(iii) and 6(iv)

4. Involving communities in the research process

Many social scientists would argue that one of the major thrusts and possibly the major responsibility of the social sciences in this country is to work for a redistribution of power and wealth and to attempt to readdress the manifest inequalities which exist at present. In most cases it is argued that this can only be done by working closely with people and not merely for them in what is necessarily a paternalistic and exploitative relationship. This view has led to calls for research to be a community based enterprise in which as many local people as possible participate, not only as passive informants, but as active members of a research team who are also subsequently consulted in the writing up and dissemination of results. This perspective was pioneered on the international scene well over two decades ago (Berreman, 1968; Weaver, 1973; Lee, 1979). Often referred to as advocacy research or, more recently in this country, as community or participatory research, much of this type of work has been done locally by community workers and sociologists (Lund, 1982; Walters, 1986) as well as anthropologists (Ramphele, 1988; Derman and Poultney, 1985) and in the AASA Guidelines we read in general that:

> As far as is possible anthropologists should try and involve the people being studied in the planning and execution of research projects, and they should recognize that their obligations to the participants or the host

community may not end (indeed many would argue, should not end), with the completion of their fieldwork or research project.

The exact nature of the obligations which extend beyond the period of fieldwork are not specified but many anthropologists, like other social scientists, become involved in assisting both individuals and the communities they study in various ways (Ellen, 1984: 138; Foster, *et al.* 1979: 343-346). In this country the latter often leads them into the field of development in advising either formal planning bodies or the community itself on economic and social reform and policy. This course of action may, in itself, lead to confrontations with yet further ethical dilemmas and often apparently contradictory ethical imperatives, some of which have already been highlighted.

The text of the AASA Ethical Guidelines which have been drawn upon in this paper is not suggested as the definitive answer to all problems experienced by social scientists in this country. It is offered merely as a pointer to the direction in which those who are working closely with communities and are concerned about these issues, might look. There are disciplines or şub disciplines, particularly those of a clinical nature, which require special provisions while, in other cases, the dominant research methodology of a discipline may not involve the close personal contact which, it is argued here, poses a potential threat to research participants. More important is the consideration of ethical concerns in research situations in which non-social scientists join the social scientist's research team. Although there are notable exceptions, the initial outlook of many of the medical, agricultural and technical personnel seconded to community services is a fairly hard line "scientific" one which has little in common with the more flexible and far less complacently assured views of many contemporary social scientists. For those natural scientists who have not previously had to consider the human factor to any great degree, caution over the ethics of close personal involvement and the responsibility to think critically about the balance of power between researcher and researched may be as new as their involvement in action and intervention research. In their case probably the most useful advise is to look ahead, expect problems and mistrust the motives or the good sense of the many gatekeepers they will encounter. It is neither paranoia nor arrogance which suggests that there may be people who will seek to use both the information and the expertise of the social scientist or community worker to their own ends and the oft quoted maxim that knowledge is power, while commonplace to most social scientists, may be usefully reiterated here (Horowitz, 1939: 606; Ellen, 1984: 134; Preston-Whyte, 1986).

Taken all in all we may well quote the advise given to anthropologists in the AASA Guidelines to the effect that researchers should always ensure that they get the terms of reference of their study clear with all concerned, that is with sponsors, research participants, colleagues and gatekeepers and that, before going into the field, they have the agreement of all concerned that it is they who retain the right to:

(a) protect the rights and interests of their research participants.
(b) make ethical decisions in their research;
(c) publish as they see fit (AASA Guidelines 1988 5(i).

If they are able to do this, they will have as free a hand as possible to decide on any problematic issues which may arise in the course of their work without undue pressure from outside or for that matter, from the inside.

5. The AASA Ethical Guidelines and the HSRC Code of Ethics compared

If the clauses quoted immediately above are read in combination with the clauses examined earlier in this paper, it will become clear that the AASA Guidelines seek not only to put the researcher in the position to be able to make all critical decisions in respect to both research and publication, but seek also to put the interests of the research participants before any other consideration, even that of what may be referred to as the "scientific endeavour" itself. In this the whole tenor of the AASA document is in marked contrast to the HSRC Code of Ethics which reflects, it is suggested, a very different perception of the role of both the researcher and of human science research in contemporary South Africa. A brief consideration of some of the main provisions of the HSRC Code and of the research philosophy which it appears to enshrine, will make this clear.

It must be conceded that the framers of the HSRC document faced the difficult task of having to contend not only with divisions of and changes in opinion within a single discipline but those across the various disciplines represented in the HSRC's research institutes. In addition the HSRC conducts contract research for outside sponsors and notably for government departments. Although a positive approach towards open research is expressed in a number of the clauses of the Code, others provide the leeway to accede to the demands of sponsors for confidentiality. Furthermore, while the researcher is given a primary role in deciding what material may be collected and how this is to be done, the balance of power between researcher and sponsor on the one hand, and research participants and the research community on the other, is clearly weighted on the side of the former. In the last resort, the choice of appropriate research methods, including the use of covert techniques, is justified on what are assumed to be objective and thus essentially incontestable scientific and/or humanitarian considerations. In this it is suggested that the full complexities of the contemporary social and political context in which much social research is conducted is ignored.

In respect to the issue of open research we read in clause 1 (3) of the HSRC Code that "In the communication of their findings researchers subscribe to the principles of honesty, comprehensiveness and openness to public scrutiny", while in clause 111(4)) it is stated that "the HSRC is committed to upholding the principle that findings should be made freely available to the public". The achievement of these ideals could, however, be compromised by the statement that "Research which is undertaken on behalf of sponsors is subject to the usual conventions of contract research" (111(1)). What exactly these conventions are is not specified. Not only is there no warning that secret reports might be the subject of controversy, but the right to demand such confidentiality is given to sponsors alone. Neither the researcher nor the community being studied are suggested as appropriate bodies with which a right of veto might be lodged. Indeed clause 1V(3) adds the rather ambiguous remark that "The HSRC recognizes society's right of access to research findings" suggesting, perhaps, that once it has been collected, research material should be in the public domain and accessible to all. We have drawn attention to the problems that this could entail.

The contradictions inherent in the clauses quoted above are glaring but, of course, are not in themselves surprising. In drawing up a formal code of conduct it is virtually impossible not to stumble on mutually contradictory values.[8] The criticism being leveled here is that this point is not made and neither are the complexities and nuances of the possible dilemmas spelled out. Although in the final analysis the AASA Guidelines may be no more restrictive, the issue of confidentiality is opened for debate in that document and researchers are sensitized to a range of thinking on the topic. The nearest approach in the HSRC Code to allowing respondents to limit or control access to research material appears in a clause designed to protect the identity of research participants [11(17)].

The topic of covert research is dealt with with a good deal of circumspection in the HSRC Code, but it is accepted that in some cases this type of research is justified on scientific or humanitarian grounds. In clause 11(4) we read

> Where methodological factors seem to demand that the identities of the researchers and knowledge of their work be kept from the participants - as in some forms of participant observation[9] and other covert methods - the following issues should be taken into consideration before embarking on the research:
> (a) whether the research is justified in terms of its likely benefits, and
> (b) whether alternative methods which do not involve deception are available.

> If covert methods are in fact used, the participants are offered an explanation whenever possible, and as soon as possible.

In respect to (a) who exactly it is who will decide on what constitutes "benefits" is not discussed, but it is presumably the researcher - or the sponsor. With regard to (b), as we have seen, some anthropologists would deny that there are any grounds, humanitarian or scientific, upon which covert research can be justified. What is again at issue here is where the power in the relationship of researcher and researched is seen to lie. Confirming the impression that as far as the HSRC is concerned it rests with the researcher and sponsor is the last comment of the section quoted above which suggests that if covert research methods are used, those affected should (but not must) be informed of this fact as soon after the fact as possible. This in fact contradicts clause 11(5) which follows it, to wit:

> Researchers respect the right individuals to refuse to participate in research, and to withdraw their participation at any stage.

How if they do not know that research is being undertaken can they object to it or withdraw?

Taken all in all the provisions of the HSRC Research Code read as a statement of belief in a fairly uncomplicated and essentially positivist social science. It is as much for this reason as any other that it fails to reflect the orientations and many of the concerns which dominate the thinking of a number of South African social scientists.There is, furthermore, no suggestion in the HSRC Code that research should be directed at any one quarter of society rather than another or that there are any grounds for giving priority to addressing the problems of the poor, powerless and those who are otherwise disadvantaged. Quite the contrary in fact. It is stated categorically that the HSRC is "committed to conducting research which will contribute to the welfare and quality of life of all South Africans." and that it "strives to serve South African society as a whole rather than any sectional interests" (Clauses 1V (1) and (2). While, at one level, this may be an important reminder to a body of practicing researchers which is largely white, educated and well off, that other communities exist which deserve research, it does not direct particular attention to addressing the problems of poverty and underdevelopment, not to mention the unequal distributions of power in this country. Nor does it suggest that elites and the current holders of power require critical attention as much as do the poor and powerless (Ellen, 1984: 138). Although it could be countered that the AASA Guidelines do not explicitly direct research attention to these areas either, two of the more controversial clauses do address these issues albeit indirectly.

6. Social Scientists and the wider society

The concern which is expressed in much of the research done by southern African anthropologists focuses in the first instance on the particular people amongst and with whom they are working. Researchers and their research participants do not, however, live in a social or political vacuum. In this country many of the people among whom research is done are poor, and in social and political terms they are disadvantaged relative to the majority of South Africans. Some anthropologists, like many other social scientists the world over, define their role in society as being not only that of thinkers and analysts, but catalysts for change. This is true as much of those who regard themselves predominantly as academics as of those who are actively involved in development research and those whose primary concern lies in promoting the technicalities of better medical, educational and other services. In situations where there is widespread agreement that certain people and sections of communities are the recipients of unjust treatment, of continuing violence or of repeated intrusions upon individual freedom, many anthropologists argue that it is not merely a personal, but a professional duty to speak out against any such situations in which they find themselves, and to which they may, furthermore, inadvertently be a party (Berreman, 1973). Where, as occurs in this country, most anthropologists are both researchers and citizens, what they find in the course of their research cannot, and on this argument, should not be separated from their experiences and responsibilities as South Africans. The ethical dilemmas which face "insider" anthropologists have received some, but by no means sufficient, attention (Madan, 1969) in the literature, but they merely add force to the type of critique launched by Berreman when he posed the question "Is Anthropology alive?" (1968).

It is basically this type of thinking which lay behind the inclusion in the AASA Guidelines of its most contentious clauses. These are that anthropologists should:

> call attention to inequalities, cases of blatant injustice, violence and intrusion upon individual freedom which they may encounter in the course of their professional activity (AASA 1988: 2(i)).

and that they should in particular

> be opposed to the system of apartheid and accept a commitment to widening the understanding and practice of democracy, non-racialism and non-sexism (AASA 1988 2(ii)).

It is above all the explicit reference to apartheid that caused most comment during the discussions which preceded the adoption of the Guidelines and which, it is claimed, have made it impossible for some South African anthropologists to join AASA. The argument of the latter is that this clause confounds the political and professional persona of the anthropologist and in this way not only "drags politics" into the academic arena, but also prejudices scientific objectivity. This is an old argument and one on which there can be no compromise. In its more general form it is by no means limited to the anthropological community in this country and nor even to the academic sphere.

The bottom line is that those anthropologists who have joined AASA have committed themselves publicly to a stand not only against apartheid but also against the state. While this may not be any more than many have already done in their private capacities as citizens, it does mean that they have seen fit to carry their opposition publicly into their professional lives. During the debate that both preceded and has subsequently followed the adoption of clause 2(ii) it was argued by those who were opposed to its inclusion that their objection was not so much to the statement against apartheid as to the fact that other totalitarian regimes are not mentioned by name and that this indicated the political nature of the Association. In some people's books this was exactly the purpose of establishing an anthropological association in this country at this particular point in time and it was felt that statements from such a professional association may carry more weight than those of a number of private (and for the most part, little known) researchers. This sanguine position has still to be proved a reality, although the basis does seem to have been laid for political action.

What clause 2(1) and 2(2) have done is to make manifest the ideological and political differences which exist within the wider anthropological community in South Africa and to which Kuper has recently once again drawn attention (1988). In a very real sense the passing of the clause condemning apartheid in the AASA Guidelines is also a response to attacks on the South African anthropology of earlier decades as a conservative and in itself politically committed discipline which was made use of to provide an intellectual justification for apartheid (Dubow, 1984). It may well be the anthropologists who have accepted the AASA Guidelines are following in the footsteps of some of their forebears (who were, however, of a very political persuasion) in believing that they have an active role to play in public life and that their responsibilities as academics and researchers are not divisible from their responsibilities as citizens.

Before the final version of the AASA Ethical Guidelines was decided upon it was suggested that a more appropriate line of action would be to include the statement against Apartheid in the Constitution of the Association. This would have taken the issue of the broader social and political responsibility of the anthropologist out of the ethical arena. This was, however, not a popular alternative with the majority of the potential membership of the Association who felt so strongly on the issue of the injustice of Apartheid that it was finally decided to include a statement to this effect in the Constitution as well as in the Ethical Guidelines. This is, of course, in keeping with moves in other academic (if not necessarily purely disciplinary) associations in this country.

7. Concluding Remarks: The limits of ethical codes and guidelines

Much of the discussion in the first part of this paper centered around the experiences of anthropologists and of the dilemmas likely to confront any researchers who work closely and intensively with particular individuals and with communities whose goodwill and trust they come to enjoy and, indeed, upon which their success depends. Implicit in the argument presented here is the view that most varieties of qualitative research necessarily involve exposure to a wide range of information, much of which could be used to the detriment of research participants. As a result certain standards of research and confidentiality are a matter of major concern. In the case of the AASSA it was felt that membership of the Association should be limited to anthropologists who by their acceptance of the Ethical Guidelines, indicated an acceptance of the corporate research standards enunciated in that document.

The motivation for raising the topic of research ethics in a multidisciplinary forum is to suggest that other social scientists may face similar dilemmas and that the issue is not one which can or should be limited by disciplinary boundaries. I would argue that all social scientists should be aware of both the personal and wider repercussions of their work and, as we make use of the research techniques developed in the different disciplines, we should also take cognizance of the problems they have been seen to bring in their wake. What is called for is open and frank discussion of these issues by those human scientists who, regardless of disciplinary boundaries, tend to make use of the same research methods, and the formulation of a document which will provide guidance for both for those entering the field and for those faced with particular dilemmas. Such a document would, in all probability, be fairly lengthy, but its value would be commensurately greater than a short statement like that to be found in the HSRC Research Code. Ideally such a document would be able to take into account various theoretical and ideological perspectives which are not considered in the HSRC Code and, if updated fairly regularly, would reflect changes in perspective which inevitably occur with time and changing social and political circumstances. Unlike a formal code, a statement of ethical dilemmas such as is suggested here, could be added to when necessary simply by the addition of further discussion on topics of concern as they arise. It would, in fact, take the form of a short compendium of advice rather than a formal or coercive code of conduct.

Footnotes

1. A distinction should be made between a formal Code of Ethics which is designed to be prescriptive and which, once adopted by an association, can be used to sanction offenders, and a set of ethical guidelines which set out accepted standards for educational purposes but which do not attempt to set up fixed criteria by which the actions of members may be judged and their behaviour penalised.

2. The use of the term Research Participant reflects well the change which has taken place in thinking on the relationship between researcher and researched. No longer is it assumed that the latter is merely a passive subject of study. In Anthropology the closeness of interaction which is built up with the people studied makes them seem not only like participants in the research, but often as hosts. In cases where the researched are involved in planning and executing the research the term research participant is completely accurate as will be shown later in the paper.

3. The references to Ellen (1984) all refer to the chapter by Akeroyd on ethics which appears in an edited volume on Ethnographic Research and contains an excellent account of the situation in anthropology, and a full bibliography on the topic.

4. The fact that ones research participants in long term field situations involving participant observation often do forget that one is "on duty' all the time and may reveal what in formal interviews they might hesitate to speak about is problematic (Ellen, 1984: 145). It is commonly referred to as "back stage research" and it must be acknowledge that many anthropologists welcome it and use the fruits of it to good effect. What is necessary is a sensitivity in the choice of how much to reveal and how to disguise the informants sufficiently to protect both their interests and feelings.

5. They are by no means unusual. An oft quoted example is that of the outcry which followed the publication of Oscar Lewis' *The People of Sanchez* (Beals, 1969: 11-15) which first sensitized academics to the feelings of people who were the subject of studies presenting many of them in what could be construed as a negative light.

6. Project Camelot was a US army sponsored research project which was orginated in the mid nineteen sixties and which aimed at investigating the

preconditions for internal conflict in neighboring countries starting with South America. Although its purpose was revealed and the work aborted before any material had been collected, the negative publicity which the iniative received was catalytic in terms of the awareness which it engendered of ethical issues amongst American social scientists. It led either directly or indirectly to the passing of the Principles of Professional Responsibility of the American Anthropological Association in 1971.

7. Action research may be defined as the attempt of researchers involved in so-called pure research to assist the communities in which they are working in such fields as promoting education, health care or skills training. Advocacy research "involves working with communities which are struggling with change agencies even if this position brings the anthropologists into opposition to the agency" (Foster, et al. 1979). Since its early days at least twenty years ago this position has advanced both in sophistication and militancy (Lee, 1979). In South Africa work with communities in the process and fight for social and economic transformation is often referred to as development research.

8. This point is made explicit in the preamble to the AASA Ethical Guidelines.

9. The assumption made in this clause that participant observation is a form of covert research method would be contested by anthropologists. As this paper suggests, it is because most anthropologists recognize, however, the potential of participant observation for clandestine research, that they demand that scrupulousness be institutionalized in respect to telling those who are observed what is happening and in deciding what may properly be revealed in publication.

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15

The relationship between science (theory) and morality (practice)

G.A. Rauche

Critical reflections on the scientific method

It may be stated with impunity that the search for truth is central to man's manifold activities. Both Plato and Aristotle pointed out that it was the sense of wonderment that made man strive for the knowledge of truth. The question was: What was the principle underlying the world, determining its structure and governing its processes? So from the very start there was a belief in truth without which man would not be searching for it in the various fields of his cultural activities, the existence of which is evidence of his quest. That he has not found it is proved by the fact that his researches continue, nor is it likely that he will ever find it. Would he succeed in accomplishing this, he would be like God, who alone is credited with knowing *the* Truth. But then man would cease to be man. It is, however, precisely when we take man as man, with all his inspirations, needs, shortcomings, weaknesses and failures [in brief with his imperfections and limitations] that we begin to understand the importance of his striving for the knowledge of truth. Such striving is of vital importance to him. It is of existential significance in that the knowledge of truth would set him free from his basic experience of conflict as a *contingent* (changing, variable) experience, due to his changing experience of reality and conditions of life.

Because of this basic conflict experience, which he shares with the other and which is universal, man constitutes his contingent experience of the world in an ever new methodological form as a theory. He does so in the sciences, the humanities, the fine arts and religion. These theories are *truth-perspectives,* in so far as they represent man's view of the world in terms of his conflict experience of it under specific circumstances of life or historical conditions. They are truth-perspectives in that they reflect the specific conflicts and needs arising from the conflict experience with which man seeks to cope in

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a particular historical situation. *In theory,* not in practice, they overcome conflict and gratify man's needs that emerge from it in a particular situation. Since they do so in theory as against other theories, these truth-perspectives remain controversial and problematic. They thus prove to be contingent themselves. By entering into a critical relation with each other, on the grounds of which the argument on truth conducted between man and his fellow-man goes on, they show their practical significance. The latter consists in the fact that they are at the same time self-projections through which man realizes and fulfils himself, even if theoretically. By projecting himself into the world, he humanizes it and gets acquainted with it. It becomes *his own* world; therefore he knows it. It gives him a feeling of certainty, security and peace of mind (happiness), which sets him free from the feeling of uncertainty, insecurity and unease, due to the conflict experience, which he seeks to overcome.

In accordance with this, all theories from whatever type of human experience they are methodologically constituted, thus yielding whatever type of knowledge, represent so and so many cycles of the human self. In constituting them, man starts from himself and ends with himself. Making use of a certain method, he constitutes his theory from his conflict experience under changing historical conditions and then proceeds to explain the world, himself and his position in the world as well as his relationship with the other (society and the state) according to the principle of identity. Man's conception of the world determines his image of himself, which in turn conditions his relationship with the other in society and the state.

All these types of projection of the human self into man's environment, which present the scale of human cultural activity and from which the purposefulness and value of human action are derived, are not only constantly changing, but occur in such divergent forms that they remain controversial and problematic. If we take the field of the physical sciences, for example, we observe that their present general approach and method are the result of a long controversial process of debate and discussion, of positing and repositing conflicting scientific theories, which, on changing premises and assumptions, were put forward only to be challenged by others. This process is still in full swing today. Problems such as the wave-particle aspect of the electron or the photon, determinism and indeterminism in atomic events, the finiteness and infinity of the universe or the question of matter, the controversy of mechanism and vitalism or the body-mind (brain-consciousness) problem in the biological sciences are still being hotly debated today. In the philosophical disciplines, there is the controversy over the origin and extent of knowledge in epistemology, the debate on the meaning and validity of metaphysics, the dispute on the validity of moral judgement in ethics and the discussion on the validity of rational judgement and the meaning of propositions in logic.

It would thus appear that knowledge of the *Truth*, in the sense of something that is what it is by itself and through itself and cannot be otherwise, is inaccessible to human understanding. Man knows only truth-perspectives that are methodologically constituted from various types of his contingent experience.

This includes mathematical and logical truths, which are method-bound and derive their validity from a specific framework of reference, from certain self-conceived norms and premises, without which they are meaningless. The perspectival and problematic character of human truths, also in non-empirical disciplines such as mathematics and logic, can be seen by comparing Euclid's geometry with that of Rieman or traditional syllogistic logic with propositional logic. The controversial nature of Russell's and Whitehead's attempt to establish certain fundamental logical axioms is well known. Apart from Russell's and Whitehead's logicism, the controversy over the foundation of mathematics produced Brouwer's "intuitionism" and Hilbert's "formalism". The former holds that the "fundamental assumptions of mathematics cannot be formulated in a definitely fixed and complete form, but are subject to continual examination and possible supplementation by intuition" (Von Mises, 1960: III, 1747-1748). The latter tries to show that the fundamental axioms are contained in mathematics itself and that

> certain aggregates of symbols are assumed as premises; these are the axioms, and from them further groups of signs are derived according to fixed rules and in a purely mechanical manner, i.e., without the use of conclusions drawn from their interpretation; the new groups are then the provable theorems (*op. cit.*: 1751).

It needs pointing out that "non-empirical" disciplines such as logic are empirical in the sense that their models are conceived from man's contingent experience of reality (historical conditions) as well. In that respect they are of existential significance. While traditional syllogistic logic, which lasted throughout the Middle Ages until the 19th century, complied with man's religious experience and the theocentric outlook and was instrumental in deriving the rational structure of the cosmos from God (Christian cosmos), modern propositional logic complies with contemporary man's functionalistic (technological) experience and interest. The world is no longer a well-ordered cosmos but consists of functional processes and interactions, both in nature and in society. Man's question is no longer: What is a thing?; What is its essence?, but: How do things function? How do they interact? Propositional logic traces the functional relations of either scientific language or natural language. This piecemeal approach corresponds to a piecemeal world.

Among those thinkers tracing the relations of scientific language, we find Frege, Russell, the early Wittgenstein and Carnap. Among those who trace the relations of natural language, there are the later Wittgenstein, Ryle, Austin, etc.

A prototype of the piecemeal world experience is Karl Popper (Popper, 1959). Together with Kuhn, Lakatos and Quine, who substituted new theories in the philosophy of science and in logic, he criticizes the verification principle of the logical positivists (Wittgenstein, Carnap, Ayer). He applies his functionalistic method to both the sciences and history (historical and social events). In doing so, he arrives at what Marcuse has called the one-dimensional man. By absolutizing the functionalistic method, he arrives at the opposite of an open society, the latter being his declared aim.

The question of the truth-value of scientific propositions emerges from Popper's principle of falsification. In order to be scientific a theory must be falsifiable (Op.cit.: 1751). To Popper no scientific theory is conclusive but liable to modification by the discovery of new facts. It is in this way that scientific knowledge expands. Popper's observation that no scientific theory is conclusive but open to change all the time is true enough, but his claim about the increase of knowledge is questionable. This would mean merely an increase of one type of knowledge namely, the functional processes of the sciences and technology. However useful when kept within their own limits and appropriate field of investigation, science and technology become destructive when absolutized. They assume control of man and destroy him as an individual and as a personality. They render him one-dimensional. In no way does such knowledge constitute the whole truth about man, who, besides functional aspirations, has also other desires, such as moral, aesthetic and religious ones. Man lives not on bread alone.

This totalitarian functionalism proves particularly pernicious when applied to history and the social sciences. In terms of his "critical rationalism", Popper rejects historical theories that conceive historical laws and trends (Popper, 1967), uncritically ignoring the question under what particular socio-political conditions such theories were developed and with which problems they sought to cope. Hence his arbitrary misrepresentation of thinkers such as Plato, Hegel, Marx and Nietzsche in *The open society and its enemies* (1945). In spite of his claim that history consists of problem situations, he fails to see that Hegel attempted to cope with the problems that had emerged as a result of the French Revolution; Marx struggled with the problems as they emerged in the wake of the Industrial Revolution and Nietzsche experienced the problems in a period of cultural decline. Popper proposes to deal with the historical problem situations in a functionalistic way: in terms of a *social technology*, which is the task of *social engineers*, to construct the open society on a scientific basis.[1]

268

In narrowing down the concept of science to the natural sciences, the functional method of which he also applies to history and the social sciences, Popper overlooks the mind's constitutive factor: that scientific knowledge is methodologically constituted by the mind from a specific type of human experience in the same way that historical, moral, aesthetical, metaphysical and religious knowledge is constituted, each type from a specific type of human experience, which differs *in quality* from another type. Just as there are different types of human experience, so there are different types of knowledge, each methodologically constituted from the respective type of experience. Science has thus a far wider meaning than Popper's narrow concept. It signifies various types of knowledge, which are methodologically constituted from a specific type of human experience. In this sense, then, scientific knowledge is just as man-made as are other types of knowledge.

The man-madeness of scientific knowledge in its wider meaning consists in its methodological character. Method, which means *the way* to reach an aim, shows science as an intellectual human activity. Being indispensable for the constitution and reconstitution of human experience as theory under changing conditions of life as well as the changing human needs and aspirations emerging from them, method is conceived as the *truth-function* of theory: thus as scientific knowledge in its wider meaning. It is method that lends scientific theory, of whatever type, an intelligible form as a *truth-perspective*.

Since scientific method is man-made, no scientific theory, of whatever type, is conclusive or final, but remains problematic and controversial. Thus scientific theories enter into a critical relationship with each other, which indicates the epistemological limit and in terms of which the dynamics of scientific argument continues. This critical relationship reflects the subjective and objective character of scientific theories as truth-perspectives. Their objective character is illustrated by the fact that, without universal understanding - the mutual understanding of different perspectives - no meaningful argument would be possible. Such continuing scientific argument in the various disciplines under changing conditions of human existence, as well as human needs and aspirations emerging from them, points to an interplay of theory and practice.

In the light of the previous observations, it should have become clear that the absolutization of the functionalist method in our time glosses over the problem of knowledge and truth. In fact, the absolutization of any method to obtain knowledge - for example the dialectical one in the various brands of Marxism - and the absolutization of any human truth by such a method is unscientific in the sense that it leads to the loss of critical insight into the limits of any method and the controversial nature of its content - the theory constituted by it. Such critical and self-critical insight is however the very hallmark of true science. The absolutization of the methods of the natural sciences and technology, as it is experienced today, leads to an uncritical cult of scientism and technologism. This results in the development of a false consciousness, an alienation from reality as contingent experience. Man becomes estranged from himself and his fellow-man. He himself becomes a mere social function and is related to the other by social functions. Accordingly, the social sciences make every effort to apply the functional method to their own field of enquiry, replacing the tracing of human relations in society by functional relations of social roles and interactions. This is evident from the writings of contemporary sociologists, such as Radcliffe Brown, Malinowski, Thurnwald and Parsons. The functionalistic approach in the social sciences means that society is seen to consist not of individuals and persons but of social interactions. Such an approach results in the loss of the philosophical content of science, which according to the eminent physicist, Werner Heisenberg (1955: 132), "is retained only if it (science) becomes aware of its limits".

We are thus confronted not only with one type of knowledge but with several types. These types are methodologically constituted and reconstituted in the form of varying theories and in terms of man's contingent experience of reality. Without such methodological constitution, no scientific knowledge is possible. It is the interplay of theory and practice in the act of constituting and reconstituting knowledge under changing conditions of life which sets in motion scientific argument in the wider sense of the word. When Max Born (1965: 87) says: "Physics is only one science among many and all science only one activity of the human intellect among many", we move a step further, pointing out that science is any kind of knowledge constituted in methodological form from man's variegated experience of reality: physical experience, historical experience, moral experience, aesthetic experience and religious experience. The theories constituted from these types of experience change along with the changing circumstances of life.

If science were defined as the methodological constitution of various types of knowledge from various types of experience under constantly changing conditions of life (historical conditions) and the needs and interests arising from the conflict experience in the wake of such change, the present one-dimensional science dogma would be overcome and man would be emancipated from it. This clearly points to the interrelation between science and morality, theory and practice.

The common root of science and morality

Unfortunately, the critical insight of the great scientists and the moral consequences drawn from it, have been lost by equating knowledge with one aspect of it: functional knowledge (Heisenberg, 1955: 132 and Born, 1965: 191-192). The vast expansion of the natural sciences in the wake of the dis-

278

coveries in nuclear physics and their functional applicability changed the scientific climate in such a way that we can speak of a *credo*, which is of moral consequence. The cult of scientism and technologism growing from it, results in man's overestimating himself. It leads to a *hubris* which, on closer analysis, highlights the unscientific nature of scientism. The latter lacks the Socratic spirit of critical questioning, the hallmark of true scientific thinking. Scientism ignores man's historical and contingent being, which calls in question all human knowledge and renders problematic all scientific theory, with the result that absolute knowledge and conclusive answers to human questioning are impossible. It becomes so evident that, by seeking to force all knowledge into the functionalistic method of the natural sciences and technology and reduce it to a value-free neutral level, scientism betrays its arbitrary and dogmatic character. Such an approach causes man to be alienated from the dynamics of contingent reality and destroys his freedom, which, on account of his contingent experience of multi-dimensional reality, issues from his critical and self-critical insight into his theoretical and practical activities. From this, it is already evident that man's contingent experience of reality emerges as the common root of science and morality.

As has already been pointed out, scientism affects fundamentally the contemporary way of thinking, in spite of all the differences as far as the current philosophical alternatives are concerned. In a certain sense, all prominent contemporary philosophical alternatives have the functionalistic approach of scientism in common. It is discernible in the functionalism of analytical philosophy, the actionalism of existentialist philosophy and the operationalism of Marxist philosophy. All three philosophical alternatives are reductionist, which also holds true for phenomenology. All these philosophical alternatives are reductionist in the sense that they reject constitutive philosophical theory. They are all functionalistic in the sense that they reduce philosophy to an analytical activity (philosophical analysis), an intentional act of consciousness (phenomenology), an existential act of self-realization in nothingness (existentialism), and an operational act of labour as an act of changing the world (Marxism). Yet, on closer analysis, these philosophical alternatives turn out to be themselves theories and, as such, constitutive. They contain world formulae through which the world is constituted, as is the case in all philosophical theories.

The world formulae that emerge from their reductionist method are: the functional relations of language, which reflect the world in analytical philosophy; the intentional act of consciousness in phenomenology, through which the meaningful world is constituted; the existential act of self-realization in nothingness, which reflects man's relationship with the world in existentialism; and the various concepts of labour in the various schools of Marxism and neo-Marxism by the functional operation of which the world is changed in such a way that man is freed from his conflict with the world, himself and society. It is by the merging of theory and practice through labour, in whatever way it is conceived in the various brands of Marxism, that society is emancipated from repression.

As far as the relationship between science and morality is concerned, it may be said that, whereas the contradiction between analytical philosophy and existentialism indicates an unbridgeable gulf between science and morality, theory and practice, the Marxist models attempt to unite these contradictories under the umbrella of dialectics. If it is however true that, as has been shown, all theories - whether scientific or moral - are always called in question by man's contingent experience of reality and are thus inconclusive, then the root of both science and morality must be found outside all theory: in man's contingent experience of reality. This thesis is confirmed by the crisis of contemporary science. Caused by a totalitarian scientism, this crisis reflects a functionalist science concept, which not only negates all knowledge of the essence of things, but also rejects all transcendental reflection. Thereby, it pronounces a value judgement. The moral consequences of such a one-dimensional way of thinking are evident. The therapeutic motive stated by Wittgenstein with regard to this analytical method, points to a metaphysical and moral dimension in his way of thinking.

Man's traumatic experience of the science crisis manifests itself in man's thinking in various ways (Gellner, 1968: 167-175). In analytical philosophy, it shows itself in the argument from impotence. In other words, it shows itself in the self-negation of philosophy, whereby the analysts hope to expose the "pathology" of philosophical language. According to Gellner, thought is not bound by the kind of language game it employs, but it is precisely in what analysts call the "pathology of language" - the continual reassessing of our terms and norms which are built into languages as a presupposition for intellectual advancement - that genuine thought consists. In existentialist philosophy, the traumatic experience of the science crisis finds expression in feelings of dread, anguish, guilt and nausea. It stands to reason that this traumatic experience may initiate a change in human thinking and so be the turning-point that causes man to become aware again of the true nature of scientific thinking and thereby true freedom. For it is the very hallmark of true science, critical and self-critical insight into man's multifarious theoretical and practical activities, that is also the hallmark of true freedom.

Ultimately, true freedom is grounded in man's authentic existence. This means that the traumatic experience of the science crisis directs man's consciousness towards the real ground of all knowledge and theory, namely, his contingent experience of reality. It is from this basic experience that all questioning about knowledge and the meaning of life originates and on the grounds of which all human knowledge remains questionable and problematic. In other words, his contingent experience of reality indicates to man his natural limits as a finite, historical and contingent being. In the light of life's kinematics, where all human thinking remains questionable, the claim of scientism to universal validity vanishes and, from its unscientific dogma, man gropes his way to a critical questioning, issuing from the existential situation in which he finds himself: a critical enquiry that also calls in question the philosophical alternatives originating from scientism on the one hand and, on the other, from the experience of the science crisis. To put it differently, authentic scientific thinking originates from the experience of man's natural limitation, changeability and variability. It is grounded in the natural conflict between man and the other, which is reflected in the controversial and problematic character of all theories.

The above observations illustrate the truth that science and morality are fed from the same source: man's contingent experience of reality. The rediscovery of this source and acting from it resolves the science crisis with all its phenomena of intellectual rigidity, dogmatism, self-righteousness and violence. This rediscovery sets man free. As a critical and self-critical personality he attains authentic existence. Instead of getting lost in one specific theory, the man of critical thinking is aware of the historical relatedness and problematic character of all theories in view of the other's critical questioning. He realizes that the realm of freedom cannot be reached by an act of absolute self-transcendence, which plunges man into an inauthentic existence of unfreedom, but is given in his contingent experience of reality. This fundamental experience makes it possible for him to order and reorder the world and environment in a meaningful way, according to the changing historical conditions and circumstances of life. Last but not least, man, by his contingent experience of reality, knows himself to be responsible to his fellow-man. He realizes that what matters is to guard, together with the other, in wise self-restraint and critical dialogue, the treasure of freedom. If anything, this is man's and his fellow-man's common destiny.

In general, the critical relationship obtaining between scientific theories and by which man and his fellow-man are referred to each other in scientific argument implies a moral *ought*. It reveals the moral dimension of the sciences (of whatever type) and the empirical dimension of morality.

Note

As far as history in general is concerned, Popper, in terms of his self-conceived principle of fertility, states: "There is no history of mankind, there is only an indefinite number of histories of all kinds of aspects of human life" ("Has History a Meaning?", in: *The open society and its enemies*, Vol 2, p. 270). In this sense he states categorically: "History has no meaning", *op. cit.*, p. 269. Also cf.

"Selbstbefreiung durch das Wissen", in: L Reinisch, *Der Sinn der Geschichte*, pp. 100-116.

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Cultural and media studies

16

Paradigms in media research

Pieter J. Fourie

The purpose of this paper is to describe the two dominant paradigms in mass communications research, namely the empirical or administrative paradigm and the critical paradigm. In doing this, I will rely on the ideas and criticism of well known scholars in this field, namely Jay Blumler (1980), James Curran and Michael Gurevitch (1982), Todd Gitlin (1978), Everett Rogers (1982), Peter Golding and Graham Murdock (1980).

The paper deals with mass communication and not with other forms of communication research, such as inter-personal, intercultural, developmental, educational or group communication.

1. Introduction

Contemporary mass communication research emerged as part of the development of mass society. The new media of this century, namely mass produced and distributed newspapers, radio, film and television were and are products of industrialization and modernization. They would not have been possible without the large audiences resulting from urbanization. Prior to the 1920s, there was little in the way of systematic investigation of the communication and particularly of the functions and effects of the mass media. There was, however a great deal being written about communication, rhetorics and media. Most intellectuals of the nineteenth century denounced the media of that time for their assumed destructive effects on the social order. It was only well into the present century that scientists became aware of the need to approach the issues of the media from an objective and value-free perspective (cf. Lowery and De Fleur). This gave rise to what is known today as the "empirical" or "administrative paradigm" in media research - a paradigm closely linked to the methodology and theory building strategies of the social and behavioral scien-

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ces. The other dominant paradigm which was to emerge, is the "critical paradigm" closely linked to the work of the Frankfurt School.

2. The empirical paradigm

Paul F. Lazarsfeld, father of the administrative paradigm, describes this paradigm as follows :

> Behind the idea of administrative communications research is the notion that modern media of communication are tools handled by people or agencies for given purposes. The purpose may be to sell goods, or to raise the intellectual standards of the population, or to secure an understanding of governmental policies, but in all cases, to someone who uses a medium for something, it is the task of research to make the tool better known and thus to facilitate its use (Blumler, 1980: 34).

The administrative paradigm is characterized by quantitative empiricism, functionalism, and positivism. It generally emphasizes the direct effects of mass communication, while often ignoring the broader context in which such communication is embedded.

The point of departure in this paradigm is a perception of the mass media as neutral tools, capable of serving a wide range of purposes. Research takes as given the purposes of media users, or would-be users, and then collects the information intended to promote the realization of those purposes. This might include studies of people's communication preferences, their exposure patterns and the various content forms made available to audiences, as well as studies of media impact, effects, influence or power under diverse conditions of presentation and reception.

In general, one could say that the administrative school sees the media as instruments (in the hands of their owners) that can and should be applied to achieve a certain goal. Research therefore focuses on the functions and effects of the media.

The functions are seen to be the provision of information, entertainment and education. Insofar as effects are concerned, the argument runs more or less that greater knowledge of the effects of the media will result in a more effective fulfilment of the functions of the media.

With these assumptions as background, innumerable studies of the effects and consequences of the media, the individual and social functions of the media, the ways in which the media are "used" by recipients (uses and gratification studies) and quantitative content analyses and market research (who is the recipient?) have been done.

The nature of the research paradigm of the administrative school is best illustrated by their studies of media effects.

During the forties and fifties these studies resulted in the so-called "bullet" or the "hypodermic needle" theory of the effects of the media.

This theory equates the influence of the media with the effect of an intravenous injection: certain values, ideas and attitudes are "injected" into the individual media user, resulting in particular behaviour. The recipient is therefore seen as a passive and helpless victim of media impact.

The main objection to this theory is that it regards the media user or the recipient as an isolated entity and fails to take account of:

(1) the recipient's personal values, norms, human relations and cultural and educational background, all of which act as a buffer against the allegedly negative portrayals of the media;
(2) the fact that people's eventual behaviour, their attitudes and notions of the world, are co-determined by several other variables such as their personal, cultural and socio-economic background.

Most of the effects studies that seem to lend plausibility to the hypodermic needle theory are in fact based on certain limited assumptions, such as:

(1) that the receiver is helplessly exposed to and at the mercy of the media,

(2) that the recipient is helplessly caught up in a (behaviourist) stimulus-response relationship with the media.

Today, however, it is accepted that the hypodermic needle theory represents a mechanistic, unsophisticated approach, deriving from the pessimism and sense of threat which the rapidly developing mass media, after the fashion of new phenomena, engendered in those early days. The research results that support this hypothesis should moreover be interpreted against the background of their time of origin - before, during and shortly after World War II, a period when people often relied exclusively on the radio for information, when there were few rival media and when the media (specifically radio) were a novelty enjoying remarkable credibility.

It was a period when social scientists were critically scrutinising existing social structures and voicing grave concern about the influence of modernisation, technological advancement and urbanisation on the spiritual and cultural development of people.

During the middle fifties, it was, however, gradually realised that, because of the large number of mediating factors involved, the reliable measurement of media effects is extremely difficult. In the late fifties a prominent communication scientist, J.T. Klapper (1958), came to the following conclusions about the nature of media effects:

(1) As a rule media communication in itself is not a necessary or sufficient cause for behavioural change. At most it operates in conjunction with and via certain mediating factors and influences.

(2) The existence of identifiable mediating factors implies that mass communication cannot be regarded as the direct cause of certain behaviour, although it can contribute to behavioural change. In this regard, mass communication should rather be seen as reinforcing existing behaviour. In fact, the media serve to reinforce, rather than change behaviour.

(3) In cases where mass communication does change behaviour, there are no mediating factors or, if there are, these factors themselves operate in the direction of change.

(4) The effect exercised by the media is itself subject to such variables as situation, *Zeitgeist* and context.

This new awareness led to a reassesment of the impact of the mass media, which again gave rise to a new academic orthodoxy in the sixties, namely that the mass media have only a very *limited influence*. Underpinning this reassuring conclusion about the lack of media influence was a repudiation of the mass society thesis on which the presumption of media power had been based. The view of society as being composed of isolated individuals gave way to a view of society as a honeycomb of small groups bound by a rich web of personal ties and dependencies. Stable group pressures, it was concluded, helped to shield the individual from media influence. In short, the conventional belief in the power of the media seemed to be demolished. Even assumptions about the nature of man and the structure of society on which the belief in media power had rested, was 'revealed' to be bankrupt and misguided.

During the late 1960s, and especially in the 1970s, the base of empirical or administrative research broadened considerably. Their earlier focus on direct effects was supplanted by an interest in indirect effects such as the agenda-setting function of the media. The emergence of a new paradigm, the so-called "critical paradigm", also gave rise to a new kind of stimulus in media research.

3. The critical paradigm

Whereas the empirical paradigm in the U.S.A. evolved partly out of the work of Robert E. Park and the influential Chicago School of Sociology, and out of other social sciences such as the social psychology of Kurt Lewin and the political science of Harold D. Lasswell, mass communication research in Europe evolved from the foundations of the Frankfurt School.

The Frankfurt School, originally strongly influenced by Marxist theory, is highly critical of capitalistic society, and, in the case of mass communication research, of the role of mass media institutions in society (many of which are organized as capitalistic enterprises). Greater emphasis is given to the social-political-economic context of communication. Critical scholars are of opinion that administrative research put too much emphasis on communication itself, ignoring the holistic context in which communication occurs. To ignore this context is to seriously distort the reality of communication. Their critique also extends to quantitative research designs and methods, such as the survey and the experiment. The opinion is that these methods are individualistic and microanalytical, and hence not capable of revealing much of social context. Critical scholars, therefore, prefer less-structured methods that yield more qualitative data.

Against this background critical scholars attacked the administrative school's limited model of media influence (developed in the sixties). The media, they argued, are ideological agencies that play a central role in maintaining class domination: research studies that deny media influence are, therefore so disabled in their theoretical approach as to be scarcely worth confronting.

The main criticism against research done in the administrative tradition can be summarized as follows:

(i) Research conducted in the administrative paradigm and the resulting theories about media effects reveals little about the media as a dominant source and transmitter of meaning. The administrative approach shows in the words of Gitlin (1978: 223), a certain:

> ... fetishism with facts, facts which by their raw muscularity, their indisputability, their very hardness take on the authority of coherent theory. The fact becomes a sort of commodity, the common currency of discourse, to be compared with, exhanged for, and supplanted by others, just as the fact as it is presented through mass media becomes authority itself, an orientation to the bewildering world that lies outside one's milieu and outside one's control.

(ii) The administrative approach neglects theory and there is an underdevelopment of a conceptual framework to guide research. Although administrative research claims to be imbedded in a theory of mass society, this theory is an implicit presupposition, rather than an explicit exposition.

(iii) In the administrative paradigm theory construction is often confused with model construction. In the words of Golding and Murdock (1980: 67):

> At the simplest level it has been identified with model construction in which a few key concepts are connected by vague generalisations, normally expressed diagrammatically. How many chain diagrams linking sender-message-receiver have been presented as insightful theories, with the occasional addition of a loop going in the opposite direction called "feedback" as a sophisticated refinement? The more advanced of these "models" have a dotted line round the edge called "social context".

(iv) The administrative paradigm is a bargaining paradigm, desiring harmonious relations among the commanding institutions, within a common, hegemonic ideological frame; in this case, that established through the legitimacy of a commercial culture industry. Administrative media research descends directly from the development of sophisticated marketing techniques. The theory of effects was first developed for the direct, explicit use of broadcasters and advertisers and continues to be used mostly in those circles. This marketing orientation takes the consumerist frame for granted, asks questions that arise within it, questions about "how", and stubbornly does not ask others. It is interested in how the mass media may increase their reach, and in how ordinary social life presents obstacles to the extension of media power. It is not interested in whether the reach of the mass media is a social good, and in which circumstances. It is not interested in the structural and cultural consequences of different models of communication ownership. It is not interested in the construction of a worldview through media techniques. Nor does it take as problematic the consumer culture itself. It cannot imagine a living political discourse that would be affected for better or worse by media representations of politics. Questions of this sort are not "practical" for the institutions that define what is practical (cf. Gitlin, 1978: 237). As Merton (1968: 505) puts it :

> The categories of [mass communications] research have, until the recent past, been shaped not so much by the needs of sociological or psychological theory as by the practical needs of those groups and agencies which have created the demand for audience research. Under direct market pressures and military needs, definite research techniques are developed and these techniques initially bear the marks of their origin: they are strongly conditioned by the practical uses to which they are first to be put.

The critical paradigm is sceptical of the endeavour of taking a single purpose and studying the means of its realization in isolation from the total historical situation in which such planning and activity takes place. It is rather accepted that modern media of communication do far more to people than even those who administer them mean to do. That is why critical communications reseach calls for the study of the general role of media in the present system. It proceeds from a broad general theory of the prevailing social trends, locating communication organizations and processes within them. Furthermore, a sense of basic human values, which are continually denied and violated by existing economic, socal and communication arrangements, is introduced into critical research.

The main thesis in the critical paradigm is that the media mold and relay dominant meaning systems which are adapted by audiences and integrated into class based or "situated" meaning systems. This thesis has contributed to a growing recognition within empirical communications research that more attention needs to be paid to the influence of the media on the ideological categories and frames of reference through which people understand the world. Evolving from the relatively limited conception of media "agenda-setting" - the ranking of issues, in terms of their perceived importance, a new interest has developed in the wider "cognitive effects" of the media.

The recognition of the power of the media as an ideological vehicle, in contrast to the limited influence thesis of the administrative school, clearly raises questions as to how and by whom this power is wielded. Answers to these questions have been sought through the investigation and analysis of the structures and practices of media organizations (cf. Curran, Gurevitch and Woollacott, 1982: 16).

According to Curran *et al.* (1982: 16) concern with the study of media institutions, their work practices and their relationship with their socio-political environment, came from theoretical developments outside the narrow confines of administrative media research.

First, developments in the sociological study of large scale, formal organizations yielded theories of organizational structure and behaviour, as well as analytical tools, which were seen to be applicable to the study of media organizations and of their work pratices and production processes.

Secondly, the increasing influence of Marxist theory, with its challenge to pluralist models of power in society, prompted a reappraisal of the role of the media in society, and focused attention on the structure and the organization of the media. The media came to be seen, in this perspective, not as an autonomous organizational system, but as a set of institutions closely linked to the dominant power structure through ownership, legal regulation, the values implicit in the professional ideologies in the media, and the structures and ideological consequences of prevailing modes of newsgathering.

Thirdly, increasing attention to the study of the role of the mass media in politics indicated the importance of examining the relationship between media institutions and political institutions of society, and the ways in which political communication emerges as a subtly composite product of the interaction between these two sets of institutions (Ibid. 16-17).

These influences resulted in mainly four strands of interest:

(i) Institutional structures and role relationships
(ii) The political economy of media institutions
(iii) Professional ideologies and work practices
(iv) Interaction of media institutions with the socio-political environment.

Researchers working in this area share the assumption that an examination of the political, organizational and professional factors which impinge on the process of message production could shed light on the question of the power of the media. Because different factors are selected for examination within each strand of study, these studies complement each other. When assessed together, they provide a comprehensive view of the ways in which media messages are produced and shaped, and offer insights into the ways in which different influences on this process are combined in a single composite product (ibid.: 16).

As far as the analysis of media content is concerned, one of the most important shifts in the critical paradigm has been the redirection of attention to the *formal qualities of media discourse;* from quantitative content analysis to qualitative structuralist accounts of the media in terms of paradigms like Saussurean linguistics, the structural anthropology of Levi-Strauss, the semiotics of Roland Barthes and Lacan's reworking of psychoanalysis.

The central concern is with the systems and processes of signification and representation, the key to which has been seen to lie in the analysis of texts - films, photographs, television programmes and so forth.

Administrative scholars utilize content analysis methods to categorize media content into quantitative data, which they can analyze to test certain hypotheses. Content analysis of mass media messages by critical scholars are usually less quantitative (they may not be quantitative at all), and more semiological in nature; the intent is often to uncover the presumed motives of the message-maker or else to provide understanding of the communication rules underlying the message. There is often less concern with objectivity, as measured by the possibility of another communication researcher to analyze the same message in the same categories.

The main criticism against these qualitative forms of content analysis is that what begins as an analysis and interpretation of a specific text, is often presented as though it is an analysis of the social dynamics of class domination. (cf. Golding and Murdoch, 1980: 70.)

One of the best examples is Dorfman and Mattelart's (1971) *To read Donald Duck*. These critical scholars analyzed the content of Walt Disney's Donald Duck comic strip, as it was published in Latin American newspapers to show that it contained subtle themes of U.S. imperialism towards developing nations. It is implied that such content may have influenced its readers. In contrast, adminstrative scholars imply such an assumption of an effect only with much caution, as the administrative scholars know of the considerable research evidence that indicates that most mass media messages do not have an effect (and hence that content does not equal effect) (cf. Rogers, 1982: 130).

However, critical scholars often carry out content analysis, not in order to infer effects on an audience, but to make inferences about the message-makers. For example, Dorfman and Mattelart (op. cit.) conducted their content analysis of Donald Duck comics in order to provide evidence about the imperialistic motivations of Walt Disney and his staff.

Here we see an illustration of the contrasting orientation of administrative and critical communications scholars:

(i) adminstrative researchers emphasize the eventual understanding of communication effects on an audience, and they conduct content analysis in order to infer such effects, while

(ii) critical scholars emphasize understanding the control of a communication system, so they conduct content analysis in order to make inferences about mass media institutions.

Rogers (1982: 130) points out that this is an example of where administrative and critical scholars could profitably learn from each other and combine forces (or at least their types of research) to mutual advantage.

One could imagine a research project in which a study of the mass media institution is combined with a content analysis of the messages that it produces, and with an audience survey of these messages' effects. For example, one wonders how Dorfman and Mattelart's (op.cit.) study might have been broadened by gathering data (1) from Walt Disney Studios about why the cartoon strip's makers utilized the themes that the content analysis displayed, and (2) from readers of these comics to determine what effects, if any, they caused. Such a multi-method approach to a research problem is usually superior to using only a single research method. Unfortunatally, there are at present few such researchers in the field of mass communication research who follow such a multi-dimensional methodology. This is also one of the main problems in South African media research.

4. Media research in South Africa

In South Africa, both at university level and at the Institute for Communication Research of the HSRC, the dominant paradigm is administrative.

Research is behaviorist, functionalist and positivist. Up to now South African scholars have not contributed anything new to the field. They follow mainly in the footsteps of their American colleagues. The emphasis is on quantitative content analysis, uses-and-gratification studies, effects, etc. Even in research fields where South Africans could play a leading role, for example in the fields of media policy in a changing society, intercultural and developmental communication and communication in multi-cultural organizations, very little of worth has been done.

One must inevitably ask what the reasons are for this situation. I will try to summarize them as follows:

1. Senior scholars in the South African field of communication and media research have up to now mostly come from fields such as sociology, social psychology, philosophy of art, philosophy, educational sciences, drama, political science and even pastoral psychology. And each of them has brought with him or her their own conception of what communication is or should be. Although this could have led to new insights and useful multi-dimensional approaches, most of these scholars have not been prepared to work together. The spirit of scholarly debate seems curiously muted. There is no attention to and discussion of the underlying assumptions of the diverse research tendencies in the process of coming to grips with what one is doing and where one stands in response to fundamental challenges.

2. With the exception of the University of South Africa, the Centre for Contemporary Cultural Studies of the University of Natal and the University of the Witwatersrand universities have up to now neglected theory and methodology. The emphasis is mainly on vocational training. The result is a curious hesitation to address in any sustained way those larger themes and controversies from which the real world of mass communications cannot be isolated, especially in the South African society.

3. Many of the research projects undertaken by the Institute for Communication Research of the HSRC will most probably not stand up to criticism leveled against administrative research, namely that it is bargaining, desiring harmonious relations among commanding institutions within a common, hegemonic ideological frame. There is hardly any attempt to explore the interface between media organizations and forces operative in the surrounding social system.

5. A multi-dimensional paradigm

Although scholars of both the schools, in Europe, the USA and in South Africa, hope that the approaches might complement each other and even be cultivated collaboratively in joint ventures, such cooperation, has, with the exception of a few research projects, not materialized up to now. This is due to a number of stumbling blocks on both sides.

Administrative scholars err mainly in conflating two sorts of purposes which they bring under the single umbrella of administrative research as if they were essentially alike. It seems as if they see no fundamental difference between research designed to promote media owners' and administrators' present goals, whatever they might be - such as selling goods, and research designed to serve some broader social purpose, to which adminstrators and owners might not be commmitted, and to which they would therefore have first to be called, such as disseminating good music, drama or mature political information to the masses. Consequently, they draw no clear distinction between research in the service of the current objectives of media institutions and research aimed to modify those objectives. (Blumler [1980: 35] traces these shortcomings to the beginnings of administrative research and the work of Lazarsfeld.)

On the other hand, much of critical research is so concerned with ideological criticism that it tends to loose objectivity in its eagerness to describe the media as a capitalist instrument for the suppression of certain social classes and political ideals.

The conflict between the two paradigms will only be resolved, and the state of media research in South Africa will only improve, once the field of media research moves away from pure description - whether it is the description of the relationships between media institutions and society, or the description of primary and secondary meanings of media messages - to a process of explanation. What is needed is an explanatory theory, theory in which empirical description and analysis support explanations of phenomena in the media. But then an explanatory theory which is not solely concerned with the ideological basis of media communication but one which is sensitive to all the variables that determine media communication. Our greatest task for the future is therefore a critical analysis of appropriate explanatory theories and in this regard we have various models in the human sciences.

An example of such an explanatory theory and model for the analysis of the media could be Erwin Panovsky's theory concerning the iconological interpretation of works of art or forms of symbolic expression.

Up to now media research has been concerned mainly with what Panofsky (1955: 51-82) would have labelled as pre-iconographical description and iconographical analysis. The appropriatness of Panovsky's theory for a media

286

science lies in the fact that in both the dominant paradigms it is accepted that the media produces symbolic forms of expression and that it is in this sense a cultural product. It would, therefore, not be too far fetched to compare the products of the media with other forms of symbolic expression and cultural products, such as the fine arts and the literary arts, and to borrow from them for our analysis of the media.

Panovsky distinguishes between pre-iconological description, iconological analysis and iconographical interpretation. Pre-iconological description is concerned with so-called objective empirical facts. It collects and classifies the evidence but does not consider itself obliged or entitled to investigate the genesis and significance of this evidence. It is purely administrative research.

Iconographical analysis, on the other hand, is occupied with motifs recognized as carriers of secondary meaning. It describes these motifs and secondary meanings but does not explain it. It is essentially, what has been called in media research, the critical paradigm.

What is necessary in media research, however, is a step towards iconological interpretation or what we might call iconological explanation. Iconological explanation requires something more than facts and a familiarity with facts and empirical data. It requires something more than an analysis of motifs and secondary meanings. It is concerned with intrinsic meanings.

To come to grips with the basic principles that underlie the choice and presentation of motifs in mass media communication, as well as the production and interpretation of media messages, we need a mental faculty comparable to that of a diagnostician - a faculty which Panovsky (1955: 64) called a 'synthetic intuition'.

Although subjective, this "synthetic intuition" must constantly be corrected by an insight and an explanation of the manner in which, under varying historical conditions, the general and essential tendencies of the human mind were and are expressed in and through mass communication. The communicologist will have to check what he thinks to be the intrinsic meaning of the media and media message(s) against the intrinsic meaning of as many other tendencies and documents of civilization: tendencies and documents bearing witness to the political, religious, philosophical and social dimensions of the object under investigation.

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17

Analyses of cultural phenomena like the "alternative media" in South Africa

P. Eric Louw

The "alternative media" is one important manifestation of the contemporary "culture of resistance" to apartheid. Given that this culture is so widespread in the townships, from Cape Town to Pretoria, and even reaching into small communities like Oudtshoorn and Howick, it is rather surprising how little attention has been paid to this phenomenon by South African academics.[1]

Researching cultural resistance and/or "alternative media" presents a number of problems around the issue of "access" to the phenomenon being studied. Firstly, the regulations imposed during the states of emergency have blocked the normal circulation of information. This has necessitated "alternative" access routes. Secondly, it is important that the discourse used by participants in the resistance is mastered if the researcher is to fully grasp what is taking place. Thirdly, a "negotiation" is called for between academic and participants to establish trust. Such negotiation will be impeded if the academic has not mastered the discourse (point two). Without establishing trust many of the subtleties of "opposition practice" will be hidden from the researcher. Fourthly, the models found in foreign textbooks do not necessarily fit local developments. Fifthly, those practicing an ivory tower and "objective" science will find it nearly impossible to gather any data from the participants in the resistance, who hold such academics in low esteem. (The criticisms one hears - and which one has to "counter" - when trying to gain "access" are that such academics are "out of touch", "exploit" the community and then leave having "disrupted" the community, but without having helped to "solve" problems.) In other words, a different approach has to be developed if one is working in this area.

For academics wishing to come to grips with this "resistance culture" a first requirement is to get out of the ivory tower and find ways of engaging with community problems in such a manner that a dialogical (two-way) learning process is created. In such a situation the academic is no longer an "expert",

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but is instead a "participant" in a social learning process. The academic may well have skills to offer such a process, but so do the other participants; and the academic skills are not necessarily the most important. This requires a re-orientation away from the more "conventional" understanding of academic intervention into situations: it requires a maximal "openness" to what is happening, with minimal "egoism" (a trait too often associated with "experts"). So an academic doing research on (and perhaps for) the resistance infrastructure would benefit by "blending in", so as to avoid alienating those with whom he or she comes into contact. (Just as an academic engaged in undertaking research on - and perhaps for - the Department of Information is advised to assume a style of behaviour and/or practices acceptable and unalienating to the bureaucrats he or she is dealing with.)

Linked to (but not collapsible into) the phenomenon of the resistance culture is the growing influence of socialist discourse (in both its Marxist and non-Marxist forms) within South Africa. Significantly, though, orthodox historical materialism is one of those models for which a neat fit will not be found in South Africa. Hence, an understanding of how Marxism and socialism are (or, in some cases, are not) influencing domestic resistance to apartheid, requires knowledge of local discourses. This represents another challenge for researchers, namely, recording and systematising the growth, and development of, as well as the various mutations in, South African socialist discourse as it has confronted local conditions. That socialists and Marxists have had to "bargain" their way into (i.e. make concessions) the various resistance organisations, has been an important element in this process.

Both these challenges - resistance culture and socialist discourse - come together where South African communication studies move to give some attention to the phenomenon of our own "homegrown" form of alternative media. Right here on our doorstep is a form of communication which is unique and is probably one of the few forms of democratic communication in existence anywhere. In this regard there is also value in paying some attention to the historical materialist approach to communication studies, because this approach has exerted a degree of influence on some elements of alternative media practice, as well as on the context within which that media operates. The changing patterns of this influence require monitoring both with regard to the effect it has on the communication produced, and with regard to influences this media itself has on the historical materialist paradigm in South Africa.

Studying "the struggle" and the "culture of resistance" requires, in part, that we recognise that a de facto "reconstruction" (mutation) of historical materialism has, and still is, taking place to enable those using this paradigm to deal more effectively with South African conditions. Historical materialist "practitioners" in South Africa have, since the 1920's, recognised the need for such "revisionism". These shifts need to be understood in terms of the changes they have wrought to historical materialism in the process. In effect, there are now many "Marxisms" in South Africa (sometimes even hostile to one another), so that using some foreign texts on the phenomenon is not unlike trying to find one's way from Pretoria to Durban using a map of Europe.

Under-documentation and under-researched processes within South Africa cannot be blithely substituted with inappropriate conceptual borrowings from the intellectual metropolis. The following developments await greater local examination: (1) the alternative media, (2) resistance culture and (3) developments in historical materialism away from an economist towards a concern also with the superstructures and the relations between the two. A group of South African researchers taking their cue from Mattelart (1980; 1983) has been grappling with issues stemming from these developments:

* Firstly, a certain discourse has developed around the struggle against apartheid. To merely dismiss it as "rhetoric", instead of trying to distill out of it the meaning of what is happening would be to miss an important research-task.

* Secondly, academics have to establish ways to gain access to the "inner-workings" of the "struggle" that is taking place. And that is going to pose a serious problem both with regard to (i) gaining access, and (ii) developing mechanisms of security to protect both the participants being studied and the researchers from security police activities.

* Thirdly, academics will require an understanding of the style and "language" of what has come to be called the Mass Democratic Movement (MDM). This MDM is an amalgam of various internal organisations (political, cultural, trade union, etc) which have adopted the Freedom Charter as their guiding principles (Swilling, 1987). This puts them into the Charterist camp together with the ANC (although, this has not ruled out "disagreements" with the ANC over strategy and tactics). The only way of gaining "real" access into what is taking place is to obtain knowledge of the "style" and "language" of the MDM.

* Fourthly, developing an understanding of the relationship between the historical materialist discourse(s) and the wider (e.g. non-socialist) MDM and ANC discourses (i.e. these mutually influence; but are not "collapsible" into each other). This will necessitate studying the "revisions" and growth in the historical material method which South African conditions have given rise to. (See for example: Wolpe, 1972; Legassick, 1974; Innes & Plaut, 1978; Erwin, 1985; and Alexander, 1985). In certain respects South Africans (albeit usually those in exile) have been at the cutting edge of the paradigm (e.g. viz Third World and African problems): this paradigm is "on the move", it is "mutating"

and "growing" in new directions (including here in South Africa). So although the historical materialist paradigm is not hegemonic within either the ANC or the MDM, it does exert some influence therein. The continually shifting developments in this regard have important implications for all of us, and hence cry out for research.

Research into resistance without grappling with the rhetoric or discourse of the South African process would produce seriously flawed results. Amongst those actively opposing the apartheid state, terms such as "the struggle" are so extensively used that they have tended to become cliched or rhetorical. Yet beneath the appearance of such cliches lies a process all too real for those caught up in a society engaged in civil warfare. Such terms need to be taken seriously and moved out of the realm of rhetoric, to be given substance, and their descriptive and explanatory power recognised. One would be unlikely to gain "access" (both in terms of understanding the process, and in terms of being granted permission by the participants to observe) without a comprehensive grasp of the discourse of the struggle. In related vein, playing the role of the distant and objective scientist would be unlikely to successfully lead to access into this field. Academic practice itself needs to be rethought in order to cope with periods of intense social polarization (see Tomaselli et al, 1988).

The point about the notion of "the struggle" is that it means different things to different people using this term. In part this is deliberate: certainly movements like both the ANC and UDF are precisely characterised by being multi-class, multiparty, multi-ethnic, multi-religious movements or alliances (Swilling, 1987). This has resulted in an MDM vagueness when it comes to policy. Yet this very vagueness has, in part, served to define what is meant by "the struggle" in the South African context: there is now sufficient substance in this notion for it to have a recognisable meaning. Those in the struggle (although they may significantly differ amongst themselves over strategy) know they are "in", and know what they mean by struggle. Those outside and opposed to the struggle are aware of its existence and their hostility to it. This is the stuff of social struggles and the conditions which often give rise to civil wars.

Many South African Marxists and social democrats appear to have thrown in their lot with the Charterists i.e. either the ANC, UDF or COSATU (although, ironically neither the aims of the ANC or UDF are explicitly "socialist").[2] Through joining these anti-apartheid coalitions socialists and Marxists both influence, and are influenced by, the other partners in the alliance. Certainly the South African Communist Party (SACP) forms one part of the ANC (the most important external resistance movement). But this has meant that the SACP has had to "background" the notion of "class struggle" through its adoption of the two-stage theory of revolution. The UDF (the most important internal resistance movement) has attracted, amongst others, many of the country's social democrats; and the Congress of South African Trade

Unions (COSATU) has provided a home for workers The state's growing repression of the UDF (especially in 1987-88), the 1988 formal (court) equation of the UDF with the ANC, and moves against COSATU (especially the 1988 "anti-COSATU" labour legislation) only served to move the various previously disparate components of Charterism closer together (see Carrim, 1987). Within the internal MDM's many constituencies, socialists hence form an important, and growing, sub-constituency, especially since this sub-constituency has its base mainly amongst the black youth which is demographically on the increase. Certainly the Marxist sub-constituency's influence on the external ANC is probably presently greater than the internal constituency it represents (although, as Davies (1988) notes, SACP influence is not as great as National Party propaganda would make it out to be).

What is clear is that socialism - in both its Marxist and social-democrat forms - has become a growing factor within contemporary South Africa, and so demands analysis. And this local discourse of socialism needs to be unraveled if its real influence on the MDM and the local alternative media is to be understood. The first thing to note is that South African socialists have adopted three different strategies for furthering their constituency's interests: (1) the (MDM) popular-alliance route (note: "popular" not "populist"); (2) the workerist (Trade Unionist) route; and (3) the (anti-Charterist) populist route (of the New Unity movement and National Forum). In terms of contemporary influence on "resistance culture" it is the former two who are really important, although this could change in the future.

The workerists - seeing class conflict as central - seek to organise a specifically working class constituency so that, if in a post-apartheid society, the bourgeoisie and petit bourgeoisie are ascendant, the working class (socialists) will have an organised voice. An analysis of the alternative press reveals some influence of the workerist voice.

The popularists down-play "class struggle" in favour of a broad anti-apartheid struggle. In a sense socialists within the popular alliance are then either not Marxists or they are de facto "practical" and pragmatic revisionists. It is this de facto (practical) revisionism which calls out for analysis: there is a need to formalize this "revision" and to theoretically "rethink" what is meant by "the struggle" for those who are "Marxists" within a (non-Marxist) popular alliance. This popular-alliance strategy, of both the MDM and the ANC, is consequently of considerable paradigmatic significance for historical materialism, because such a strategy may be pragmatic, but it significantly *alters* Marxist theory: it means that the South African (popular) "struggle" cannot be considered a Marxist (i.e. class) one, although it contains significant proletarian, and Marxist voices. One cannot understand either the "alternative press" or the "culture of resistance" outside of understanding these dynamics, and the continually shifting patterns of alliances that underpin them. Failure to understand this leads to the type of conclusions reached by Groenewald *et al.* (1988: 235 and 249) which do not account for the way in which the media serving the MDM could assist in compromise and negotiation and lead to a non-racial pluralism.

A popular struggle is above all else premised upon alliance-building, and that implies communication for co-ordination. Further, if socialism's influence is to be expanded within the MDM (obviously what local historical materialists would be seeking) it will require that this constituency develop effective communication in the sense of: (1) learning to use the alliance communication network to bargain the best possible position for itself; and (2) developing rhetorical skills to "sell" socialism to others in the alliance. Yet communication is an area that is poorly developed (relative to other areas) within Marxism. In this regard too, the South African experience has revealed a need to *reconstruct* historical materialism: i.e. since communication has not been a "traditional" area of concern for Marxists some revision and development of the historical material paradigm would seem to be called for, i.e. South African Marxists will need to "reconstruct" their paradigm to enable it to deal more "effectively" with: (1) South African popular-alliance (multi-class; multi-ethnic; multi-religious; etc) politics; and (2) communication and communicative "struggle". Because the South African popular struggle contains an imaginative socialist caucus, monitoring the extent and pace (and possibly some failures) of such "reconstruction" will hence be a necessary part of any analysis of the alternative media and the resistance culture it mediates. Monitoring and documenting the socialist caucus within the MDM alliance will require not only an understanding of the MDM, but also a sound grasp of the Marxist paradigm and the epistemology of the "reconstruction" to it. Such a "reconstruction" does not need to start from scratch because certain Marxists made important moves in the direction required when they considered the issues of ideology, superstructure, culture, etc.

Some might raise the argument, that if the Marxist method requires reconstruction to deal with South African conditions it must be flawed. Hence, why bother to deal with it? The answer is that Marxism cannot be ignored or wished away; it has acquired a role in South Africa because: (1) of the National Party (NP) government's continual anti-communist and anti-Marxist rhetoric. This has boosted the prestige of Marxism amongst significant sectors of the population (although this influence does not always take the form of a rigorous Marxist theory and practice). (2) The SACP has become an important exiled ally of the ANC, hence making it a factor that cannot be ignored within even the popular (i.e. non-Marxist) "struggle". Like both the ANC and the MDM, Marxism is now an incontrovertible part of the South African milieu. (3) The black working class is growing into a demographically central factor in South Africa, and it is this group which is, in particular, being increasingly influenced by socialist and Marxist rhetoric (especially because the government has said it is "bad", which in South Africa has acted as a catalyst for this discourse). Within the South African popular alliances (i.e. MDM and ANC) both the black proletariat and (now rooted) Marxist theory can be expected to play a growing role. (In part this is because conditions of racial-capitalist exploitation and repression continue, resulting in such theory finding acceptance.) If alternative media journalists succeed in providing a mirror of the section of society they serve, then an analysis of the discourse in the alternative media will, in part, provide a barometer of the extent and nature of such Marxist and other socialist theoretical influences (as well as the influence of the other discourses like nationalism, liberalism, Black Consciousness, etc.). But this will require an understanding of the subtleties and nuances of the various inter-meshing (and mutating) discourses within the MDM.

Further, for historical materialists, South Africa has presented a particularly promising context within which to hone and improve their methodology (both theoretically and practically). So although South Africa does not present a classic class model of revolution, it does offer: (1) a revolutionary environment; (2) a society which is industrialised and which consequently does contain a significant proletariat, and what is more (in the current context) a demographically important proletariat with considerable revolutionary potential; and (3) *because* of the problems of applying orthodox Marxist theory and practice to South Africa, this makes of South Africa an opportunity for the *development* of the historical materialist method; i.e. working out solutions for the South African anomalies would de facto expand the scope and explanatory power of the resultant (reconstructed) historical material method. Because rhetorical orthodoxy and strict class conflict models may actually serve to reduce the role which will ultimately be played by Marxists within a popular alliance (i.e. by alienating other members of the alliance), pragmaticism has demanded that a reconstructed Marxism has been required in South Africa. The specific form in which conflict and turmoil is being manifest in the local context (i.e. where class conflict and non-class conflict intersect in various configurations) poses methodological and theoretical challenges to historical materialists, and practical challenges to Marxist politicans and trade unionists. It poses both theoretical and practical challenges to communicologists employing the historical material paradigm. The way in which Marxists deal with these challenges during the contemporary period of flux will determine how much influence socialists gain (or fail to gain) in a post-apartheid South Africa. Researching their moves in this regard offers an obvious area of concern for local academics.[3]

South African Marxist analysis needs to account for the theory and practice in which the contemporary conflict may move; i.e. (1) "armed struggle", which, in the absence of negotiation, escalates to higher and higher levels of violence between the two main protagonists which are emerging, i.e. NP and ANC; or

(2) a process of negotiation between the two blocks (underpinned by a degree of violence). Understanding the reconstruction that has, and is taking place, would be a prerequisite before a reading of the alternative media could provide a picture of the shifting patterns of alliance within the culture of resistance. Such developments have and are taking place. They require documentation and research because they represent one part of the context within which the alternative media operate.

Further, for a society at war with itself, like contemporary South Africa, the Marxist concern with struggle and conflict is able to produce insights and notions which offer analysts some important explanatory tools. At the heart of this Marxist notion of struggle lies the dialectical method. Along with Lukacs (1971), this writer sees this method as the theoretical core of Marx's historical materialist approach. But although this core may prove theoretically helpful for those engaged in the South African struggle, the notion of dialectical conflict itself needs revision within the contemporary South African context: there is a need to develop a class *alliance* understanding of conflict, which Lukacs did not do. Practically, the popular revolution in Nicaragua could influence the process in South African, despite Mattelart's injunction against this (Mattelart, 1986:8); while theoretically, Poulantzas (1979, 1982) offers a number of insights as to the sort of direction historical materialists may move in once they have opted for this pluralist approach. It will be interesting to monitor if, and how, this process unfolds; and if it influences (or fails to influence) resistance culture and the alternative media.

Work at the Contemporary Cultural Studies Unit [Durban] is directed towards investigating the reconstruction of historical materialism as it affects communication and the media in South Africa. (Tomaselli & Louw, 1989). For anyone trying to get to grips with the reconstruction of the historical material paradigm such that it can deal with communication, a number of areas of concern suggest themselves, such as: the potential that reading Gramsci and Poulantzas together holds for explaining the nature of a popular counter hegemonic-alliance (as is emerging in South Africa); the role of ideology (as developed by Lukacs, the Frankfurt School, and Gramsci); the notion of ideological "co-optation" (as derived from the Frankfurt School); the notions of cultural resistance and of the popular culture of subordinate classes (as developed by the culturalists, the Birmingham School, the CCSU and the Wits History Workshop); the "struggle for the sign" (as developed by Volosinov and Tomaselli). Within the South African context, merely, trying to document (and keep pace) with the reconstructions and with the various shifts in influence of the above theorists on the socialist caucus (caucuses?) within either the MDM or ANC would be a massive task in itself. At the CCSU it is considered a necessary task because such analysis provides an important part of the wider context within which to understand the alternative media in South Africa.

Research into this area could look for: (1) the extent to which a reconstruction of historical materialism (RHM) was taking place in South Africa; (2) if so, what was the nature of this RHM, and what extent such an RHM dealt adequately with communication; (3) whether any developing RHM served, or failed to serve, in furthering the interests of either the local socialist caucus, and/or the rest of the anti-apartheid alliance; and (4) the influence, or otherwise of an RHM within the MDM alliance.

In researching a South African RHM's attempts to deal with communication and RHM's success (or otherwise) within the MDM, the following five areas seem the most obvious:

* Rigorously considering the place of communication and the media (i.e. the superstructure) within the historical material paradigm. This would entail an examination of the epistemology of the central (and often problematic) Marxist work on the "superstructure", ideology and culture. Epistemologically, the following are seen as the most important: Lukacs (1971), the Frankfurt School (e.g. Adorno & Horkheimer, 1979), Gramsci (1971), the culturalist (e.g. Williams, 1963, 1965), Althusser (1971), the Birmingham School (e.g. Hall et al, 1980), and Volosinov (1973). But there is a need to go beyond epistemology, and to consider also the methodological implications of an historical material turn to the superstructure. One can point in this regard to Habermas's (1979, 1984) proposal that reconstructing historical materialism, such that it incorporates - in a non-reductionistic manner - a concern with communication offers a way to theoretically develop and improve the historical material paradigm.

* Has such an RHM moved beyond the Habermasian (1979) theoretical challenge to consider as well the Enzensbergian (1974) practical challenge to develop a "praxis" communication? A number of themes for research suggest themselves here:

A "communicative battle" has already begun between the NP-hegemony and the MDM. This is an obvious area for documentation and research. There is a need to unpack both communication strategies. That the NP places so much emphasis on Beaufre's (1965) concern with psychological and communication struggle within their counter-revolutionary programme is an indication that they consider this to be one of their weak areas (Graaf, 1988). This being the case, the anti-apartheid alliance will presumably take a cue from the NP and develop a systematic communicative counter to Beaufre's WHAM (Win Hearts and Minds), and the related oil spots strategy (see Boraine, 1988; and Phillip, 1988). With regard to a message (within such a communicative campaign) the MDM will presumably have greater success viz the majority of the black population than any WHAM-programme could ever expect to have. But how they go about it -and the NP counter attacks - will be most interesting to observe.

* However, a South African RHM will need to deal with the MDM's need to move beyond the above message and consider (a) other potential allies for the MDM (see Jodac, 1988) ; and (b) ways of communicating with different sectors of the population. There would be a requirement to develop rhetorical communication skills, i.e. one would expect to find some effort to diffuse sophisticated media and communication skills throughout the MDM. In this area the NP is currently better placed than both the MDM and ANC because the ruling hegemony has in its control a wide range of highly trained, professional communicators (journalists, public relations officers, advertising specialists, media technicians, media bureaucrats, etc).

Expanding the MDM alliance would also require developing every communicative avenue available, including the development of an MDM media network. Such a network is already partially in place, although it has been "damaged" and forced to "mutate" by government action against it under the States of Emergency. The CCSU has already begun the process of researching this network, state action against it and its "mutations." It seems likely that this network will continue to "mutate" as a result of government repression. But although the NP-hegemony can, and will, attack and damage this network (and force it to mutate into new forms and genres), it seems unlikely that the state will be able to entirely suppress this phenomenon. (Certainly similar phenomena in Rhodesia, Vietnam, Nicaragua, Philippines, etc could not be suppressed). Also, the MDM communicative network shows signs of expanding into media other than print. So the CCSU has a lot of work cut out for it.

Building a popular alliance has required engaging in a struggle with the apartheid sign system, or what Tomaselli et al (1987) call the "struggle for the sign". The first anti-apartheid group to recognise the importance of such mobilization through "signs" was the BCM during the 1970s. However, the BCM mobilized exclusivist (racial) signs and in this way (i.e. by restricting their appeal) ruled themselves out as a serious (widely-based) counter hegemonic force. Building an anti-apartheid counter hegemony will further require a set of integrative signs which can serve to unify the various constituencies of the struggle. During 1988 both the Freedom Charter and Mandela took on the characteristics of such unifying signs. More will presumably be required, which one assumes is why the ANC, for example, has now established a cultural bureau. Because this cultural work will require that "cross-class" signs be developed. It will again be interesting to monitor how a local RHM deals with this challenge (which it will have to do if it is to remain influential within the

MDM. Traditional inter-cultural communication methods cannot deal with this phenomenon. Rather, a reconstructed historical materialism is required.

* The need has already arisen to develop (both theoretically and practically) a communication model for dialogue (i.e. democracy) as the basis for the maximization of MDM alliance-building. The best example of this would be the *Grassroots* model (Louw, 1988), in the period prior to the State of Emergency repression. The MDM will, however, presumably need to invest more effort in this area, especially if a democratic model of communication is to be successfully developed under a State of Emergency. On a theoretical level, Habermas (1974) will presumably prove useful in this regard. On a practical level the experiences in Chile (Mattelart, 1983: 356- 376), Nicaragua (Mattelart, 1986), provide useful practical examples of "communication" as "democracy-in-action". Whether the MDM successfully develops and uses such communicative and organizational practices or not will have a major impact on any future relationships between the MDM (who have stressed the need for such "democratic practices") and the ANC (for whom clandestine effectiveness has been more important). If the MDM is successful it could have a significant "democratic" impact on the final form a post-apartheid society takes.

* To be successful, the MDM will a need to understand and to counter the NP-hegemony's propaganda (tied up with the state's WHAM and "total strategy" concepts), as well as to understand how these strategies tie into the workings of state censorship and laws which affect the media. The more the NP-hegemony succeeds in "blinding" (to alternatives) and "militarizing" white South Africans (Tomaselli & Louw, 1987; and Graaf, 1988) the greater the resistance the anti-apartheid counter-hegemony will face. The NP appears to have developed a particularly clear understanding of the role of propaganda in the furthering of its goals. This clarity was demonstrated by the revelation of the Broederbond's "Masterplan for a white country: the Strategy" (Wilkins & Strydom, 1980: 280-282. Further, the capacity for NP nationalist and conservative propaganda to "confuse" and even win for the NP allies amongst the black underclasses cannot be underestimated. For example, by creating homeland infrastructures it is conceivable - following Andersons' (1983a) theory of nationalism - that tribal nationalisms could ultimately be created by the NP. It would be interesting to document and research the unfolding of the MDM's communicative strategy to try and counter this.

Conclusion

Researching the South African alternative media and the growing cultural resistance to apartheid requires a thorough versing in the above issues and problems. This particular form of communication research cannot even begin until one has mastered the "rhetoric of resistance"; gained the trust of the participants in that resistance; and grasped the intricacies of how a popular alliance works (as well as the inter-meshing discourses of liberalism, nationalism, socialism and Marxism within such an alliance). In this regard there is a need to examine the de facto (rather than imagined) influence of Marxism on both the MDM, the ANC and the cultural resistance to apartheid. To do this means, amongst other things, coming to terms with the historical material paradigm; and the revisions of this paradigm produced by South Africans who have subsumed themselves into a non-Marxist alliance.

Footnotes

1. Local academic work concerned with analysing the resistance has been largely confined around a few "nodes" of activity e.g. the Association for Sociology in Southern Africa; and the journals *Africa Perspective, Critical Health, Critical Arts* and *Transformation*. Kotze & Lourens (1988) and de Kock (1988) have been recent exceptions to this general pattern.
What is striking in these examples is the difference between Swilling (1988) and Kotze & Lourens on the one hand, and De Kock on the other hand. The first three authors have obviously made some attempt to master the discourse of the resistance, and have either negotiated direct access, or use the route of monitoring resistance writings (e.g. *Work in Progress)*. De Kock (1988) in Van Vuuren et al largely misses the point of what is happening, and notably uses foreign theoretical texts on revolution as his point of departure, instead of either (i) coming to terms with the South African phenomenon itself, or at least (ii) showing a familiarity with the research done in the area by local radical academics . In similar vein, Groenewald et al (in Marais, 1988) makes no distinction between the right and left wing presses, despite significant differences in structure, journalistic practice, etc. Had either the available work on the phenomenon been consulted, or the phenomenon itself actually been studied, such a collapsing of two such clearly different journalistic practices into one category would not have suggested.
2. However, some Marxists (notably the "purists") have joined instead the (non-Charterist) New Unity Movement or the Marxist Workers Tendency.
3. Developments in South Africa in this regard are, in many way, similar to those contemporary processes in Nicaragua. The South African developments

cry out to be documented and analysed along the lines of Mattelart's (1986) work on the Nicaraguan situation.

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18

Becoming appropriately modern: Towards a genealogy of cultural studies in South Africa

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Introductory considerations

Cultural studies is the catch all name for those forms of expression which participate more or less self consciously in the process of creating the values and terms by which their patron cultures and societies are experienced and judged. One way they do this is by affirming the values of their society, and in so doing underwrite the incessant process of cultural domestication not only of the communal id (Freud's necessary repression) but also of alternate and transgressive cultural forms and impulses (Marcuse's surplus repression). Alternatively, they might engage the dark side of the dominant culture, in an attempt to rehabilitate that which piety, routine and the civilizing process occlude. In so doing, they strive to adopt a critical stance towards this culture. This stance can take the form either of a purely negative critique, or of an affirmation of cultural alternatives to the dominant culture, usually locating the affirmation within a larger emancipatory project. More rarely, it can do both. Cultural studies, in both its descriptive and normative moments, exhibits a tension between the critical and the affirmative, the negative and the positive.

An important body of theorists will argue that the social conditions which could have supported the development of an affirming and emancipating ethos have collapsed in post-industrial societies (Adorno, Lyotard amongst others). Another more optimistic group will claim that cultural studies can, indeed must, "develop a ... concomitant language of possibility" to a "language of critique" (Giroux, 1988: 156; see also Laclau and Mouffe, 1985: 190). We will attempt to show that cultural studies oscillates between the optimists and the pessimists in South Africa as it does elsewhere. Though our paper cuts a wide swathe through the historical terrain of cultural studies in South Africa, we aim to offer no more than a preliminary outline of its general contours. Very little

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work has been done on the origins, travels and impact of international cultural theory on the South African humanities, and virtually none has been done on the development of indigenous theory. We sketch the beginnings of a genealogy of cultural studies in South Africa in terms of a) shifts in epistemological assumptions and explanatory priorities; and b) of the theoretical and historical context within which it developed and its frequently paradoxical relationship to that context.

The indigenization question

Modern cultural studies is the result of the ceaseless hybridization of different disciplines across different continents, with frequently bizarre results. We are tempted to paraphrase Marx via Edward Said and say that when a theory repeats itself elsewhere, it flirts with farce. Intercontinental theoretical "traveling" has much to do with the increasing globalization of knowledge, and it has led cultural studies as an interstitial intellectual field to be promoted by what Bourdieu (1984) has called the entrepreneurial activities of "cultural intermediaries". The mobility of cultural entrepreneurs and their historically contingent preferences does not on its own account for the lineage of South African cultural studies. To say that South Africa is a country whose economic and political development is a function of neo-colonialism is to suggest something also about the way in which cultural theory has taken root and has come to operate in this most divided of societies. Like other cultural goods in such societies, cultural studies has come in complex ways to resonate with market trends at the colonial metropoles.

This has meant that cultural theory, like other capitalist consumables, has been thrust on the South African market, and has been taken up often with scant regard for its appropriateness and articulability with indigenous cultural practices. It is not surprising then that the call to indigenize cultural studies is periodically made (eg. Masilela, 1988). In its extreme form, the indigenization argument maintains that theories and approaches developed at the metropoles are irrelevant to South Africa. This critique is, in many respects, hard to refute especially insofar as it recognizes that theory, like other forms of writing, has an intimate relation to the very particular structures and struggles of a society and arises out of them if it is not to become parasitical. What the argument tends to underestimate, however, is the extent to which South Africa is a modern capitalist state like others, and the extent to which South Africa has, in its own way, moved along the path of modernity. In doing so, South Africa has unevenly acquired the hallmarks of a modern capitalist society - an industrial economy, mass education, a consumer-driven market, and increasingly too, the formal and informal ideologies associated with such a society. To deny this is to over-homogenize the older capitalist societies on the one hand, and to over-particularize South Africa on the other. Once this is done, the indigenization argument becomes compelling, straying perilously close to a sort of geographical and hence theoretical apartheid (see Berman, 1988: 167).

What the indigenization question does alert us to is the danger of trying to periodize South Africa's path to modernity and beyond in a fashion derived too closely from the experience of the older capitalist societies. The point is not that cultural theories from such societies are inappropriate to South Africa so much as that they have a different purchase in this partly similar and partly different society. We will try to show that with respect to those societies not only is South Africa's periodization scrambled but that theories dominant elsewhere can become oppositional here, and some theories can become effective both long before their time has properly arrived, or long after their time could reasonably be expected to have ended. As Russell Berman (1988: 171) has commented regarding South Africa's progress towards modernity "for now the lesson from South Africa has to do with the antinomic *simultaneity* of the modern and the post-modern" (our emphasis) and, we might add for the sake of completeness, in some respects the pre-modern too.

We are suggesting that the travels and travails of cultural studies in South Africa is a complex product of international market trends, the globalization of knowledge, the organization (or otherwise) of cultural intermediaries and their access to the structures of South African cultural life, as well as the problematic non-linear path of modernity in South Africa and the social struggles that have helped shape its course. Our intention in what follows is not to be comprehensive, nor to offer exhaustive examples or even necessarily the best examples. We will present a schematic history of what we see as the major theoretical trends in South African cultural studies, and their forms of evolution and of transformation. Our purpose will be to show the particularly South African versions of international trends, and to highlight the curious paradoxical features that these occasion. In other words, we wish to examine how theory "traveled" to South Africa and what it did when it got here.

Modernity and dominant culture

Various writers have attempted to characterize the "dominant cultural logic" (Jameson, 1984: 57) or core cultural assumptions of industrial society and maturing capitalism. For the Frankfurt School, it is "enlightenment thinking", that form of rationality which dissolves a moral view of the ends of society into a technical emphasis on the means for infinite improvability; for the radical conservatives (like Bell, 1976 and Lasch, 1984) it is the idea of the autonomous self-determining individual; for the neo-Nietzschians (like Lyotard, 1986) it is the contraction of the will into the infinite, the insatiable desire for the new, for the unknown, for money, for power, a portrayal of modernity that echoes those evocative words of the Communist Manifesto "All that is solid melts into air" (p. 7).

However we may define it (and precisely how does not matter here), there is broad agreement that what drives the multiple discourses of modernity, that what is both its animating ideal and explanatory principle, are all variants of what we more familiarly may call *liberal humanism*. We will use humanism here as the family term for a wide variety of cognate assumptions taking individualism and human agency, improvability and rationality, justice and truth as their starting point for constructing accounts of culture and society. Humanism in one or other of its manifestations is also the principle target for the construction of critiques of modernity. Indeed, the three major movements of cultural critique in the 20th Century - the Frankfurt School critique of enlightenment thinking, the anti-humanist assault of radical structuralism, and the postmodern critique of the modern - all in different ways begin their critiques with one or other definition of this cultural dominant (see Wexler, 1987: 134). It is also fair to say that these three theories of culture have had most to do with stimulating critical cultural studies as a recognizable academic domain. The rest of this paper will attempt to outline the destiny of the various humanisms which we distinguish as liberal, radical and democratic - and the destiny of the two anti-humanisms - conservative and radical - that constitute the field of South African cultural theory. Though crucially important, the special case of Africanist humanism will not be dealt with here.

Conservatism and beyond

The dominant cultural theory in South Africa, insofar as it underpins apartheid and the material and institutional forms of its enforcement, can be said to be Afrikaner *conservative cultural theory* (hence forth, CCT). We do not mean to suggest a homogeneous conception here which would present an idealized unity of both the theory and of Afrikanerdom from within CCT. The continuing and substantial differences within CCT had increasingly to do with the emerging differences of interest between the different class forces in the Nationalist alliance that were never coherently roped together by anything approximating cultural hegemony (Posel, 1987). Nevertheless, even at times of bitterest dispute, various core themes served as at least temporary unifiers. It should not be surprising that most of these themes derived from a critique of liberal humanism.

The first phase of articulation of CCT occurred during the late 19th century via a sacralised reconstruction of Afrikaner history into what Dunbar Moodie (1975: 15) calls a "civil religion". This civil religion, compatible with a narrow republicanism, was essentially an exclusivist populism that united a simple version of "the Afrikaner people" against three perceived evils - imperialism, capitalism, and egalitarian liberalism.

The first properly theoretical strain to leave its imprint on CCT was a form of Calvinism brought back by Dutch Reformed theologians returning from Holland, having been trained in the Kuyperian tradition. This deepened the populist ideal, but, by distinguishing between different spheres of civic life, managed to separate the political jurisdiction of government from cultural and religious spheres which were to be subject only to the sovereignty of God. Kuyperianism thus stimulated a separate cultural politics and, in common with Calvinism elsewhere, inspired a mass of voluntary associations - debating societies, womens associations, youth groups, dramatic societies, agricultural co-ops and so on, which began the job of actively proselytizing in favour of a more affirmative national cultural conception of "the people". As *Die Republikein* said in 1935: "Culture is the name given to the common spiritual possessions of a People". This conception hardened into a fully fledged affirmative and self-conscious "struggle to maintain our language and culture" (van Rooy in Moodie, 1975: 111) compatible with a not illiberal form of nationalism. It took the intense organisational work of the Broederbond in the 1940s to link this to a common political idea.

Before that could effectively happen, another strain brought back by returning intellectuals in the 1930s - neo-Fichtean nationalism - made an impact on CCT. Especially in the writings of Diederichs, essentials of a nationalism consciously opposed to universalist humanism was developed. This nationalism takes the principle of ethnic or national unification on a spiritual plane, making it a higher principle than local sovereignty of cultural and political spheres. This paved the way for an all-inclusive ethnic nationalism that overrode local jurisdictions and paved the way in turn for a political solution to schismatic Afrikanerdom.

The 1940s was the time of developing organization of Afrikaner workers, of Afrikaner capital, and of the Afrikaner petty bourgeoisie. The rhetoric consequently became more pro-capitalist, (in the name of a share for "all the people"), more anti-communist (in the name of anti-atheism and anti-materialism), and more anti-liberal (..."modern democracy has been falsified by liberalism to depravity and impotence") (L.J. du Plessis quoted in Moodie, 1975: 226). At the same time, colour entered the discourse of CCT in a far more intensified way than before, a product of an exclusive Christian nationalism which saw organically unified socio-economic-political entities in national-racial rather than populist terms.

The instability within the ruling alliance continued to bubble up in the form of ideological disputes after the Nationalist victory of 1948. With the Broederbond going from strength to strength in the 1950s, "by 1960 the ascendant constituent of the nationalist alliance was a rapidly expanding group of more sophisticated, assertive and educated white intellectuals and professionals" (Lazar, 1987a: 22). It was amongst these intellectuals that the next battle around the moral-cultural ground of apartheid was fought. The theoretical debate of decisive importance was conducted within SABRA between the neo-Fichtean descended radical or vertical separationists (or "visionaries" as Lazar calls them: Olivier, Sadie, Tomlinson, Gerdener,van Eeden) and those in the Kuyperian line of descent fortified by pragmatism, who argued for cultural and political segregation but economic integration (Verwoerd, Eiselen and increasingly, the Broederbond itself) (see Lazar, 1987b). The visionaries were impelled by a sharpened moral vision with exclusivist populist roots; the pragmatists were impelled increasingly by a politics of the workable. With the victory of Verwoerd over the visionaries, the moral dimension of CCT was finally displaced in the construction and consolidation of apartheid. The 19th century populist dream was also gone outside of periodic nostalgic appeals by ultra rightists harking back to a pre-capitalist" imagined community" (Anderson, 1985). Henceforth, the pragmatists could only advance into technicism, which is where a new breed of Afrikaner intellectual, in greater or lesser flight from high CCT, went from the late 1960s onwards. We return to this point later.

The abiding paradox of CCT is that, its underpinning of apartheid notwithstanding, it exhibits in a number of respects all the hallmarks of an exemplary cultural theory. It had:

1. A critical dimension consistently defining itself against liberal humanism;

2. An affirmative dimension, with a utopian vision, a positive set of values to strive for, and, until the 1960s, a primarily moral basis for collective action. It had, in other words, affirmative content.

3. A clear constituency which it could address;

4. An intimate understanding of the relation between culture and power;

5. A philosophical and rich popular dimensions. It was during its heyday continuously engaged, debated and nourished across the spectrum of theoretical and popular discourses by the gamut of Afrikaner institutional life;

6. Flexibility for mobilization on both local and national levels. It was in this sense an empowering discourse.

In retrospect, the cultural conservatives were doomed from the start. The individuation essential for operating in a developed market society and within capitalist relations of production could not be resisted forever, nor can the issue of universal individual juridico-political rights appropriate to this social form (see Turner, 1986: 11). The flaw at the heart of CCT was that it was based on a nostalgia for a pre-capitalist social collectivity which the advance of capitalism and its evolving cultural accompaniments has everywhere smashed (see Turner, 1986: 2). That it realized this at the start in its opposition to liberal humanism lends CCT also a tragic dimension.

Humanism in opposition

Liberal humanism, always part of the intellectual scene in South Africa, has mostly played in a minor key. Unquestionably the dominant ethos in most of the capitalist countries of Europe, the UK and USA during the late 19th century and much of the 20th, the South African "liberal tradition of discourse about truth and justice has proved highly ambiguous and more often than not abortive" (du Toit, 1988a: 22). From the 19th century Cape liberalism of Stockenstrom, through the Oxford lectures of Smuts in 1907, to the Phelps-Stokes lectures of Hoernle in 1939, influential writers otherwise liberal found it difficult to "step outside the assumptions of segregationism" (Legassick, 1976: 237), despite an otherwise humanistic universalism which was consistently opposed by CCT.

Political liberalism never crossed over very effectively into the cultural domain in South Africa either. David Maughan Brown (1984) has shown that a number of politically liberal novelists - Cope, van der Post and Bloom amongst others - failed to take distance from the unconscious of their time, and unwittingly reproduced the obsession with barbarism that underlay segregationism. In literary or political publications, very little of note was established. *Coming of Age* in 1930 drew little attention and *The Forum*, first published in 1938, though greeted with optimism by Paton and others (Paton, 1965: 228), hardly touched South African cultural life in the way that *Koers* and other mouth pieces of CCT did. As one contributor to *The Forum* complained: "English publishing facilities for poets and novelists in this country are practically non-existent. The English press must come in for blame. In the first place, the standard of literary criticism,except in highbrow and highly intellectual papers, generally leftist, is exceedingly low" (*The Forum* 9 September 1944; quoted in Haines, 1987: 4).

In that the 2nd World War betokens to many the prevailing of liberalism over conservatism, it provided hope that liberalism might henceforth have a greater impact on South African affairs, a hope which proved in the event of the 1948 Nationalist victory to be sadly misplaced. The quintessentially liberal activities of the National Union of South African Students and the Black Sash aside, it was only in the later 1980s that a debate around liberalism re-appeared (du Toit, 1988b: 24). Given that South Africa is a reasonably evolved capitalist society, and given the dominance of liberal humanism in most other capitalist countries, its lamentable showing in South Africa requires some kind of explanation.

Liberal humanism has consistently found itself in a paradoxical position in South Africa. On the one hand, it is part of the dominant world discourse and, especially in the wake of Milnerian reconstruction, occupied a quasi-colonial position in South African intellectual life, a fact clearly perceived and bitterly resented by the proponents of CCT. Many liberal intellectuals evinced an unconscious (or not so unconscious) superiority as a result, which most probably also accounts for the lack of purchase of a distinctive South African liberalism in the popular mind of the electorate, despite attempts by some writers like Hoernle to develop one. "South Africanism" equally did not fire the popular imagination. On the other hand, at least since 1948, liberal humanism found itself in opposition to the emergent CCT and therefore spent much of its energy on defensive appeals to a kind of universal humanism.

Important is another tradition of humanism in South Africa, one which Legassick (1976: 239) calls democratic rather than liberal, and which was nurtured during the early decades of this century within the Communist Party (SACP), the unions and the African nationalist organisations. Although the two humanisms at times sought common cause - the SACP campaigned for Smuts' candidate in Turfontein in the 1924 election for example (see Roux, 1972; see also Lewsen, 1987) - the two increasingly found themselves at odds, with writers like Hoernle explicitly taking issue with the implications of democratization. There was also a strain of liberal humanism more avowedly integrationist and democratic, but it never managed to occupy the liberal high ground, and indeed, existed uneasily between liberal humanism and democratic humanism. The argument emerging in the war years that industrialization would liberalize race relations was a direct liberal response to the challenge of democratization. Indeed, the liberals have now as then often spent more time attacking their left rather than the dominant CCT, "a sad project" as Stadler (1987) has remarked. The Kliptown Freedom Charter of 1955 is one of the finer moments of a universalist South African humanism, containing as it does echoes of the American Declaration of Independence, the 1941 Atlantic Charter, and the democratic rhetoric of the anti-fascist coalition (see Berman,1988; see also Luthuli, 1962: 98). The Charter's vision is an affirmative one and nothing in liberal humanism comes close to its mobilizing and empowering potential. For liberal humanism, the distant threat of socialism in the Charter was enough to leave it awkwardly proclaiming a conditional universalism against both left and right, pretty well defining itself into irrelevance in the process as the state, underpinned by CCT, sought to eliminate the democratic tradition. Liberal humanism has thus, since 1948 at least, been a defensive and oppositional discourse in retreat, cocooned in its superior powerlessness mainly in the English speaking universities.

Up to the early 1970s, humanist commentary on contemporary culture in South Africa, such as it was, happened largely in departments of literature, anthropology, history, drama, and education. In so far as it was critical in any way, it attempted to discuss culture as an important but benign object of study and as an instrument of evolutionary cultural progress. Any talk of cultural struggle was absent from this discourse. CCT was treated with distaste as a kind of aberration, or not at all. The theoretical styles in most of these departments were in almost every case directly derived from Britain. In addition, liberal humanism was at this time underpinned by a tradition of Anglo-American analytical philosophy in the English speaking philosophy departments, a relatively uncomplicated cultural borrowing which helped to foster the belief that the liberal humanists were no-nonsense, clear-eyed purveyors of rationality in an irrational world.

By far the most acute tradition of humanist commentary was the anti-elitist "practical criticism" approach of F R Leavis, I A Richards and the *Scrutiny* group, imported to South Africa by Edward Davis in the 1960s. Davis was an irascible anti-conservative and non-conformist. At the University of South Africa he exercised a substantial influence on literary studies till then monopolized by Arnoldian conservative humanism as exemplified by Guy Butler of Rhodes University. This cultural liberalisation made good sense in the boomtime of the swinging Britain of the 1960s, providing a sceptical but finally non-threatening foil to the expanding British culture industry. In South Africa, practical criticism always had a sharper oppositional edge. Nevertheless, the edge was at least partly vitiated by the fact that the Great Tradition that it taught, anti-elitist though it was in the British context, was in South Africa a colonial import that engaged not much at all with local culture. The result was that it never developed a critical or affirmative ground for a local politics of cultural opposition.

As a cultural theory liberal humanism has the following features:

1. It concentrates on the political rather than the cultural, the intellectual rather than the popular.

2. It has no notion of the relation of knowledge or culture to power.

3. Its colonial legacy of cultural domination somehow weakened the force of its opposition to CCT.

4. Its mobilising potential has always been limited.

Even at the moment that practical criticism in its opposition to both English and Afrikaner cultural conservatism in South Africa was taking root, a next moment of cultural theory was being prefigured in England. This was to unravel into sharply divergent strands. In his richly ironic way, Edward Said (1983) has shown how humanist cultural Marxism "traveled" to Cambridge, leaving its militant and insurrectionary pretensions with Lukacs in Budapest and its tragic vision with Goldmann in Paris. It became with Williams in Cambridge, a goad for Leavisite English studies that was becoming institutionalised, complacent and impervious to its complicity with the dominant culture. That Williams has made little impact on South African cultural studies beyond the passing reference remains something of a puzzle. But that that other, anti-humanist strand of the new wave of Marxism - Althusserianism - did, is an enigma of epic proportions.

Anti-humanism (briefly) rampant

The classical Marxist analysis of South Africa was nurtured during the middle part of this century from within the SACP. The newer generation (Wolpe, Legassick) developed the theory further in exile and back home in the early 1970s. The critical moment of this analysis was directed against the industrialization argument of liberal humanism, and against liberalism in general. The affirmative moment consisted in the theoretical assumption that the working class would be the major agent to usher in the new society. In this respect, classical Marxism in South Africa was a form of humanism, related to but not identical with the democratic humanism of the Charter tradition. If anything, this analytical Marxism avoided the issue of culture in that its account of apartheid reality was reduced to the economic. Cultural artifacts and processes of all sorts - from the writings of ideologues, to novels and state policy - were not taken seriously as shaping forces. With the fierce state repression of the Charterists from the late 1950s onwards, democracy became both politically unfeasible and theoretically unpopular. Consequently it was not the strain of radical humanism that re-emerged in the 1970s alongside the resurgence of worker militancy and organisation.

Along with British and European academics, many South African scholars rediscovered Marx via Althusser in the 1970s, although the importation here was somewhat delayed. At the metropole, the ground was perfectly prepared for a radical, anti-humanist pessimism that could articulate the crisis of confidence of the European universities, despair at endemic industrial slow-down and at the corresponding quiescence of the working class. Althusser's theory was a principled cry of frustration at the failure of Stalinism and a theoretical account of why a revolution, at least as led by the working class, seemed impossible. The main point was that without corresponding revolutions on the political and ideological levels, economic transformation alone is futile (Craib, 1984: 153).

The economic, political and cultural climate in South Africa in the middle 1970s could not have differed more markedly from that at the metropole. The economy was reaching the end of an extraordinary boom period produced by an economic policy of "racial Fordism" (Gelb, 1987) and a political policy of savage repression. There was then no liberal public sphere to cushion or mediate the polarization between capital and the state on the one hand and the mass of exploited and disenfranchised black people on the other, another index of the failure of liberal humanism. However, 1973 saw the beginnings of a rise in worker and popular militancy which continued unabated to 1985 when the revolution was being predicted by scholars and militants alike in terms of months rather than years. It was in this climate that Althusserianism inexplicably took hold, first amongst historians and historical sociologists who sought explanation for the quiescence in popular resistance of the 1960s, and later, by academics in education, literary studies, drama and media studies for whom the strictures of working in a conservative institution were in all probability more compelling than the massive wave of popular resistance building in the world outside.

The effect was paradoxical. Education is a case in point. As in sociology and history, the path to radical critique lay through an attack on liberal humanism. This meant casting doubt on the common sense category of race as an explanatory one for South Africa's ills, and foregrounding capitalism and class as the villains of the piece.

The heyday of Althusserian educational theorizing occurred between 1978 and 1984, a period of major rejection of Bantu education through a series of struggles the ferocity of which remains unmatched in any regime of mass formal schooling. Nowhere else for example, has there been a countrywide extended boycott of formal schooling; nowhere else have schools become sites for such grim and serious battles between student militants and the national army; and nowhere else has a secondary school student cadre of militants - the comrades - come to lead, if only rhetorically and by example, a national struggle for liberation. Through a large part of this, educational theorists continued to stress the reproductive moment of education, the ideological encapsulation of school children into the web of domination, and by implication, the futility of any kind of action against the system this side of revolution.

Three upshots of the situation are worth noting. The student militants swallowed the critique not only of Bantu education but also of capitalism whole, transmuted it into ringing slogans - "freedom now, education later", - and put them to use as grounding for their implacable oppositional activism. Althusser would, we think, have been astounded. The black and white trainee teachers who encountered this theory in their university courses either became frustrated with a critique which explained to them their complicity with the system of domination and the futility of trying to change it, or became immured into apathy. For the theorists, a radicalism which left nothing to do on the educational plane because it had been evacuated of all possibility on the theoretical plane, provided good justification for political inactivity.

The first response came from anti-radical humanists, liberal and conservative alike, indignant at the pessimistic repudiation of the possibilities for promoting change, instant defenders of a moderate middle-ground that barely existed, feeling beleaguered as never before by a perceived totalitarianism of the left as well as by the more familiar one from the right. As ever, this defensive response was unable to connect to an affirmative ideal and with an increasingly shrinking constituency to address.

Another radical humanism was also emerging to tackle the anti-humanism, one inspired by a certain reading of the theorist Antonio Gramsci. South Africa of the 1970s provided a particularly receptive field for the application of a humanism that stressed the possibilities rather than the impossibilities of political will and the manifold cultural forms for its deployment. The concept of cultural resistance became a ready optimistic antidote against anti-humanist pessimism. Indeed, early examples (eg. Koch, 1983) overemphasized the liberatory aspect to the degree that they were resisting Althusser. Nevertheless, radical cultural critique was rejuvenated overnight, and a host of studies made their appearance demonstrating the ingenious and resilient way in which traditional cultural forms - like music, drama and dance - could be "refunctioned" in the service of a symbolic subversion of dominant cultural forms.

Gramsci had in the 1980s another effect in South Africa, one more directly strategic and, in parallel with democratic humanism, aimed at promoting, developing and bringing into being an alternative oppositional culture, although what this could be was nowhere programmatically defined. From the late 1970s, cultural workers began to join co-operatives and worker projects with the aim of stimulating cultural expression, especially that of theatre, around intense working class experiences like strikes and court cases. Many of these cultural workers moved to the opposite end of the spectrum from anti-humanism, eschewing theory for a more immediate, concrete and practical cultural liberation in the making. For them, all forms of cultural commentary not aimed at facilitating oppositional cultural construction were regarded as one or other kind of scholastic indulgence.

They would have been singularly unimpressed by the general tone of defeat that South African anti-humanism had imported wholesale from Paris, and that informed the entire Western Marxist tradition of which Althusser was such a good example (Anderson, 1985: 42). In this they joined hands with the classical Marxists and labour scholars who would have been similarly unimpressed by the negative prognosis for the working class. Interestingly enough, the rejection of Althusser by the radicals was mostly bolstered by reference to another historically disjunct cultural import, E P Thompson, and the terms of debate here were often epistemological as they were in Britain.

In retrospect, the gross dislocation between theory and historical events aside, South African Althusserianism probably underestimated the historical role that labour was still to play, and overestimated the question of ideology. Nonetheless, it had considerable virtues for cultural studies which were not properly appreciated by its opponents.

These include the following:

1. It consistently defined itself against liberal humanism. As such, Althusserianism and associated forms of anti-humanist structuralism mark the second moment of international cultural critique after the Frankfurt School, paving the way for the third, post-industrial moment.

2. It attempted to develop a formal explanatory account of society and culture without sliding into either descriptiveness or technicism. The dispute concerning what level of abstraction is appropriate to what form of the account of the social is one of the more serious misunderstandings on the left between the Althusserians and the anti-anti-humanists.

3. It places the effectivity of the mental and of culture back on the agenda albeit in a negative form and not as an affirmative possibility.

In the end Althusserianism in South African cultural studies was undermined internally - by its own denial of both emancipatory agency and affirmative possibility - and externally, by South Africa's peculiar path to modernity that placed the working class at the forefront of social struggles just as it was slipping into historical insignificance at the metropoles. Nevertheless, along with CCT it represents the only other sustained positioning of cultural commentary in a relation of critique to the cultural dominant of industrial capitalism, namely liberal humanism, to have made an impact on South African cultural studies.

Beyond conservatism and anti-humanism

By the early 1980s, the residual CCT theorists must have been bemused by the way the radicals were fighting each other. Coming as they did from a theory of affirmation with an identifiable constituency, they would have understood Western Marxist pessimism as an acknowledgement of the failure of Marxist theory *tout court*, in that it seemed to surrender all possibility of mobilisation. Similarly, in the absence of a positive cultural programme, they would have understood the optimism of the resistance theorists as an abstract optimism without vision or content and hence no more mobilising than the abstract discourse of truth and rights in liberal humanism. For the conservatives, thus, and for the state security policies they could yet influence, it was still classical communism that was the main threat. Insofar as other radicals were seen as a threat, they too must in the end be communists. That the entire debate, not within cultural studies alone but within national and international social and political theory in general has thus passed the state apparatus by so completely unnoticed is astonishing.

As we mentioned before, the pragmatists, seeking to move further and further away from the immoral consequences of CCT's vision, had found some basis for certainty in one or other scientific, positivistic or technicist construal of the social. For them, the universalist promise of neutrality offered by these construals must have seemed a huge relief after the particularistic and unchallengeable verities of CCT. Especially after the victory of Botha reformism within the state in 1977, the various state departments bent on gradual reform all came increasingly to need detailed technical information which state-established research institutes began to provide. For these technocrats, probably not as insensitive to the theoretical debates as the securocrats, the newer radical writing that was being received with such acclaim internationally was also in the end useless, in that it had no prospective or predictive dimension. The Afrikaner pragmatists were increasingly joined by liberal humanists who at last saw the possibility for setting some of the future agenda for South Africa. For this newer breed of South African social scientist, sincerely trying to build the knowledge and expert-driven road to the future, neither the conservatives nor the radicals were seriously part of the solution.

Not for the first time in South Africa's history will the radicals and conservatives have shared a common theoretical opponent - this time of technicism - for partly similar and partly different reasons. Conservatives see technicism (or pragmatism) as anti-cultural insofar as it eschews the moral ground of Christian nationalism, indeed of any moral ground, as a basis for generating policy prescriptions. In this sense too, it can't be affirmative since, as a discourse of means, it largely avoids talking about ends. Without a basis for generating ideals or affirming a moral base, pragmatism can never be a mobilising or empowering discourse and will of necessity be limited to an ever growing technical elite. Indeed, for the radicals, technicism is a disempowering discourse insofar as it progressively precludes the basis for democratic participation in policy considerations. To this extent, radicals will see technicism as complicit with domination, in contrast to technicism's own understanding of itself as neutral.

It might thus seem a paradox that many Afrikaner intellectuals have left CCT, an affirmative albeit elitist culturalism, and one that acknowledges the relation between knowledge and power, for a neutral-claiming scientism that is without any critical, affirmative or mobilising possibility. But equally it should not come as too much of a surprise; the organic intellectuals of CCT aimed to address and empower "the People": the technical intelligentsia, here as elsewhere in maturing capitalism, aim to address and empower the state. They have forsaken one form of social integration for another. Facing the reality of state reform policy aiming to differentiate the class structure of South Africa even further, they have collectively positioned themselves to underwrite and perhaps even direct that form of integration appropriate to maturing capitalism - incorporative state rule.

On the right, technicism has elicited a renewed attempt by conservative writers like Boshoff to rethink a moral-national solution for South Africa. This will have some purchase on the consciousness of those white class strata not being advantaged by the state reform programme, but unless the path to modernity is seriously deflected in South Africa, its base of appeal will gradually shrink.

On the left, the mid-1980s saw a resurgence of interest in and mobilisation around the democratic humanist Charter, this time opposing a technical state reform policy instead of a contending ideology of nationalism. Prior to the founding Congress of the people in 1955, common sense demands were collected by an army of 50 000 Freedom Volunteers (iVolontya) and distilled into the Charter. These initiatives are to have given the Charter a certain democratic origin. There has been some debate as to whether that process was really democratic. However, the process of campaigning, electing delegates, and the Congress itself succeeded in generating a mobilising discourse that was and is quintessentially non-racial, humanist, democratic and universalist.

The Charter supposes and addresses the various constituencies of "the People" with an undifferentiated notion of culture. This is largely true in the 1980s as it was in the 1950s: in the words of Mbuli "for us culture must be a weapon that we use effectively to raise the awareness of the people about their living conditions and the reasons for their sufferings" (Suttner and Cronin, 1986: 139). In this respect it is similar to the undifferentiated populist logic of 19th century Afrikaner civil religion. The differences are more marked than the similarities however. Unlike the populism of CCT, and incidently unlike the Black Communalist populism of Black Consciousness of the late 1960s and 1970s, the Charter prefigures a modernist populism, not an anti-modernist one. On the one hand there is in the Charter an explicit acknowledgement of the plural interests created by modernism and class differentiation (as well as the residual effectivity of pre-modern ethnic and tribal interests); on the other hand, given this reality, unity can only emerge from a cultural and political democracy of the most wide-ranging sort (see Lekota, 1986).

The Charter and the campaign around it is thus a classical modernist emancipatory movement. It is underlaid by a cultural theory that has the following features:

1. A critical dimension consistently defining itself against South Africa's version of "reactionary modernism", i.e., apartheid (see Herf, 1984), a centralising and totalitarian form of modernism.

2. An affirmative dimension, a utopian vision, and a positive and universalist morality.

3. It makes an explicit appeal to every group as well as to every individual in South Africa ("...the Charter is authoritative because the people were fully consulted as to its contents and everybody, including the Afrikaners, was invited to the People's Congress" (Lekota, 1986: 198).

4. It could be said that it has a greater popular than intellectual dimension with key notions like "culture" "democracy" and "nation" theoretically undifferentiated. However, in that the core concepts - like democracy - are already intellectual rather than organic notions, it may be just as well that the intellectual strata has not run too far ahead of popular understanding, repeating the mistake of liberal humanism.

5. The potential for mobilisation on specific issues is great and it is clearly an empowering discourse.

It remains to be seen whether this form of democratic humanism is able to mobilise different interest constituencies to form an enduring unity. In order for this to happen, it will have to develop a cultural vision with greater content, and it will have to develop concomitant political and economic visions with both intellectual and popular appeal. In the end, its greatest strength is openness, its reliance upon democracy. In this respect, it points beyond South Africa's skewed modernity towards the future and is not simply bent on completing the modernist project. ("The universalist modernity of the Charter turns out to be already postmodern in the sense of presuming plurality and heterogeneity" Berman, 1988: 171). By the same token, the greatest danger facing democratic humanism is that it may become bent in the direction of a centralised, homogenizing and hence reactionary modernity served by an unaccountable technical intelligentsia. It should be clear that, if this happens, the Charter project would follow most tragically and paradoxically, in the footsteps of CCT.

Concluding comment

The purpose of this paper has been to begin a process of examination of the path of cultural studies in South Africa. For writers like Bauman (1988), modernity and its complicit cultural theories can only be soberly examined from a vantage point removed from the centre of the modernist project, from a point somewhere near the boundary, or end, of the modern era. As we have seen, South Africa has not unproblematically moved through modernity in nearly the same linear fashion as the First World capitalist countries have and our cultural writings have been correspondingly hybrid. This conclusion is itself, Bauman and Berman would argue, a postmodern one. We have begun to delineate a place where a critical examination of cultural studies can begin, while still inconclusively within an incomplete modernising movement, but with a sense perhaps of the nearing of the end to a major cycle. It is our hope that this paper will contribute to the preparation for South Africa's own cultural rupture, and perhaps, by analysing the movement towards it, hastening that process.

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Discourses and power

19

Is Foucault's project a nihilistic one?

Carlo Frigerio

> It is no longer possible to think in our day other than in the void left by man's disappearance. For this void does not create a deficiency ... it is nothing more, nothing less, than the unfolding of a space in which it is once more possible to think (Foucault, 1970: 324).

In these days an increasing uneasiness is spreading throughout our culture. It affects almost every discipline and every aspect of our intellectual life. In the medical and legal professions, in sociology, in psychology, in criminology, in science in the educational institutions, there are many signs that the deep assumptions and commitments that have shaped our thinking are today being called into question. There is a growing sense that something is wrong with the way in which the relevant options and issues are posed - a feeling that in some way we must change the categorical structure and pattern within which we think about man if we want to escape the increasing possessive and manipulative power of society. Many critics have felt that, in order to change today's orthodoxy, we should revise the conception of man which we have inherited, via Kant, from Descartes. The weakness of their criticisms is that they do not offer a clear and convincing explanation of how such conception is, in fact, the foundation upon which our present social order is built. Foucault's great merit is that he has indeed given such an explanation. He has shown that today's social structure is shaped and justified by that "knowledge" about man which is produced by the human sciences. But, Foucault says, the human sciences, as we know them today, were made possible in the first place by the Kantian conception of man. Their "scientificity" is a myth that can be exploded by breaking the Cartesian straightjacket that still dominates our philosophical reflections.

Foucault's starting point is a radical rejection of the traditional humanist view of man. In no way is he the first or the only one to have questioned the notion of the Cartesian subject, but his peculiarity lies in his asking a simple

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question: "Has 'man' always existed?" In *The Order of Things* he answers this question by showing that 'man' is a recent invention. It is at the end of the 18th century, during the epistemic rupture between the classic and the modern period, that man, as we know him today, makes his appearance. After Kant, man is no longer a being among others seeking to understand the objects of the world; now man has a "double" character. At one level he is the subject who produces knowledge, at another level he is the object of such knowledge. The 'man' who emerged from Kant's metaphysics of the subject, Foucault (1970: 318) says, is a strange "empirico-transcendental doublet", he is a being such that he is able to acquire knowledge of what renders all knowledge possible. This double aspect of man made the birth of the human sciences possible. On one side there are the "traditional" sciences - economics, biology, linguistics - which study the laws of production, biological laws, and the structure of language. On the other side the human sciences seek to abstract from them a unitary object - man - and to make of him the object of their study. Foucault argues that the human sciences are "illegitimate" sciences because they have no genuine object to talk about, the two-faced subject/object they claim to study being a recent invention which is already on its way out. But in our time the Kantian knowing subject has became a sort of categorical apriori so that the claims made by the human sciences have assumed the powerful status of "scientific knowledge". The human sciences claim to discover, in fact, Foucault says invent, new "truths" about man. They define norms, standards, deviances and pathologies, and allot treatments to them. A plethora of experts have emerged who dictate how human beings have to be measured against the norm, and then classified and disciplined accordingly; in Foucault's words "made subjects".

Foucault says that the knowledge produced by the human sciences was coupled with power creating new architectures, prisons, schools, barracks, factories, educational buildings, and new institutions where the findings of that knowledge could be applied and increased. Foucault's analyses of the genealogy of modern medicine, legal systems, psychology and criminology reveal that epistemology and politics are ineluctably intertwined in "power/knowledge" technologies and in apparatuses *(dispositifs)* by which human beings are made subjects and treated as objects. At this point Foucault makes a fundamental break with tradition. Consistent with his rejection of the notion of a Kantian knowing subject, he refuses to see the working of power/knowledge technologies as a form of repression by which some identifiable group organises the lives of other people. Writing his stories of the institutions in which knowledge of the human sciences is embedded, Foucault rejects the traditional appeal to underlying agents and claims that the emergence and the working of power/knowledge apparatuses are "autonomous". In this "autonomy thesis" many critics have seen a form of nihilism. Dreyfus and Rabinow (1988: 87) see in it an "extreme" form of nihilism "that would seek to explain, and thus explains away, all meaningful interpretation as a form of illusion". The charge is serious and should it be proved sound, that is, should it be proved that Foucault supports the idea that "everything goes", his project would lose its potential power to generate a new way of thinking about man that could lift us out of the trap of our historical categorical framework.

Let us first clarify what the charge of nihilism consists of. Dreyfus defines nihilism as "a way of life in which all values have the same value, everything is equal, or to put it in another way, there are no meaningful differences" (Dreyfus, 1980: 4). In the conclusion of their book on Foucault (1986: 205) Dreyfus and Rabinow pose a number of questions in the form of dilemmas "between a return to the traditional philosophic view that description and interpretation ultimately must correspond to the way things really are, and a nihilist view that physical reality, the body and history are whatever we take them to be". It appears that, for Dreyfus and Rabinow, the only way open to Foucault to escape nihilism is a return to some form of humanism rooted in the traditional notion of the Cartesian ego. But because they are themselves still caught up in the "traditional philosophic view" they cannot see that, if nihilism consists of "a way of life in which all values have the same value" Foucault is not a nihilist. In what follows, I will argue that Foucault escapes the charge of nihilism simply because he offers certain guidelines as to how we can make meaningful choices. Although these choices are not based on any 'objective' interpretation of the "way things really are", as his critics would like, they are none the less ethically compelling at least in the original meaning of ethos, well expressed in Heraclitus's fragment "Man's *ethos* is his *daimon*" (FR: 119).

As we have seen, Foucault's critics have focused their attacks on his thesis that human beings are made subjects by power/knowledge technologies independent of human control. For this reason, following Jana Sawicki (1987), I will begin my defence by discussing in some detail Foucault's account of how power/knowledge technologies emerge and work in our society.

It is in *Discipline and punish* that the notion of "disciplinary" technologies is clearly formulated for the first time. In that book, Foucault shows how, with the birth of the prison, a new form of power, "disciplinary power", emerged which takes the form of a technology of the body. This technology, Foucault says, requires a "knowledge of the body which is not exactly the science of its functioning" (1979: 26), but rather "knowledge of each inmate, of his behavior, his deeper states of mind, his gradual improvement; the prisons must be conceived as places of knowledge about convicts" (1979: 249). "Disciplinary Technologies" are both a form of power and a mode of knowledge. That is, power/knowledge.

The "autonomy" of disciplinary technologies is reflected in three aspects of Foucault's account. Firstly, power and knowledge are not separate entities. The power of a technology is not derived from the application into practice of a pre-existing theoretical knowledge. On the contrary, as the paradigmatic example of the Panopticon shows, knowledge takes the form of technical control. Secondly, the power exercised through power/knowledge, far from being merely repressive, is fundamentally productive; it produces "more obedient", "more useful" human bodies. A series of techniques for training the body together with techniques of surveillance, examination, documentation, comparison, measurement, and classification increases the standardisation of the population and facilitates the discovery of the "abnormal" which, in turn, becomes the subject of further scientific knowledge. Power/knowledge technologies proliferate, amplifying and justifying themselves and so producing objects of knowledge for the human sciences. Again a paradigm of how power/knowledge technologies produce objects for the human sciences is offered by the Panopticon, a "laboratory of power ... [in which] knowledge follows the advance of power discovering new objects of knowledge" (1979: 194). Finally, the third aspect of the thesis that modern technologies of objectification are beyond human control is Foucault's refusal to use traditional categories of the human subject in his analyses. The reason for his refusal is that the main feature of the humanist subject is that it is a "constitutive" subject, while Foucault's goal is to expose the process through which human beings are constituted as objectified subjects (epistemological, moral and empirical). Humanism hides the operation of power/knowledge technologies behind notions like "human nature" and "human progress". Further, humanism places the subject in a central position creating the illusion that man is the creator of his social reality, while man is in fact, according to Foucault, "dispersed" and "decentred" in it with no, or very little, access to the mechanisms that create that reality. The humanist conception of man is the underlying rationale of those ideologies of "human progress" and "human reform" which give theoretical justification to the technologies for the domination of the human body and "soul". In *Discipline and punish* (295) Foucault says: "This disciplinary technique exercised upon the body had a double effect: a "soul" to be known and a subjection to be maintained". It is in the concept of the "soul", i.e. in human consciousness, personality and subjectivity that the human sciences - psychology, criminology, psychoanalysis and sociology - are rooted and developed. "Thus, the humanity which the human sciences aim to understand and to liberate through self-understanding is itself the effect of techniques of subjection that preceded it" (Sawicki, J.: 1987).

Foucault rewrites the history of "humanistic reforms" as stories of power/knowledge technologies without any humanistic assumption about man. There is no doubt that he places these stories beyond human control. However, in order to evaluate the charge of nihilism, the point to be ascertained is whether Foucault's autonomy thesis leaves us without any possibility of choosing our course of actions on the basis of "meaningful differences". Should Foucault be a holist, attempting to provide a general account about the "essence" of technology in our society and to prescribe an anti-humanist ontology, that conclusion would probably be justified. But Foucault analyzes "particular" phenomena in which the working of disciplinary technologies has been obscured by the traditional account of human progress and reform. Like Nietzsche, he is skeptical of unreflected acceptance of traditional values and looks at their "genealogies" in order to *denaturalise* them by showing how they have been constituted. He does not propose a diagnosis of modern technology as a whole but rather of "particular" practices within it. More important, he proposes no diagnosis of the overall direction of modern technology.

Raichman (1985: 100) says that Foucault "problematises a *portion* of our current thought by discovering its historical lineages". Foucault's historical writings show how our present is rooted in a past which contained the possibility of alternatives and was hence open to radical change. Foucault's histories are designed to make the present seem less inevitable. Showing the arbitrariness of past practices he wants us to understand the arbitrariness of our present practices. Foucault's hope lies in the contingent aspect of history. Beyond disrupting the conventional operation of the metaphysical concept of man, Foucault aims to disrupt the normalising practices of the human sciences. For him human freedom does not lie in the discovery of some essential feature of the human situation, as Sartre claimed, but rather in rebellion against the arbitrary ways in which we are already defined, categorised and classified. Here we must be careful not to confuse Foucault's "rebellion" with that of the Marxist critical theory. Foucault tells us that the "history of the present" shows possible alternatives to objectifying practices, whereas the Marxist tradition aims to show how *socialist* change can be actualised in our history. Foucault does not ask us to hope for utopia, or a perfect form of life, but rather to imagine a time so different as to make our own time seem arbitrary, as past forms seem arbitrary to us. Another point that differentiates Foucault's thinking from Marxism is his conception of power. Marxist theories describe power as being rooted in and sustained by the economic structure of the society, by modes of production. Foucault develops his "analytic of power" by relying neither on the notion of the subject nor on the assumption that there are universal structural relations between power and other domains of society. For him, power is an explanatory category but explanations need not be causal, and he explains the working of power in modern society without attributing it to any conscious agency or to any underlying force like mode of production. Foucault feels the explanation can be found by mapping the historical evolution of various power relations which are spread throughout the whole network of social relations. Any social relation, he says, involves a form of power relation. A further difference between Foucault's and the Marxist conception of power is that the latter describes power only negatively - as domination, coercion, manipulation, in short repression. Foucault (1980: 59) says that the Marxist theories of power give an exaggerated role to the notion of repression. He says that power would be a fragile thing if its only function was to repress and censor. If power is strong it is because it produces knowledge. The exercise of military and educational disciplines has produced physiological and organic knowledge about the human body. For Foucault, the reduction of power to repression is not only inadequate, it is dangerous. It makes us think that the liberation struggle can be won by demasking "truths" that have been made invisible by the mechanisms of power. These truths are generally associated with a presupposed true essence of man. The idea that by unveiling them we will free ourselves from the oppressive effects of power is part of the humanist ideology. For Foucault there are no hidden truths concealed under "false" beliefs and engendered in us by those in power. Beliefs cannot be cut off from the social practices in which they are embodied, nor can beliefs be measured against a given reality existing outside those practices. Incidentally Foucault here is close to Wittgenstein's (1953: 241) claim that agreement in judgement means agreement on what people *do* and say, not on what they *believe*.

Foucault says that power, far from preventing knowledge, produces it; thus there is no reason to assume that power/knowledge practices cannot have beneficial effects. It is just that Foucault does not focus on such beneficial effects because his aim is simply to convince us to be on our guard against the dangerous effects, and thus to be able to do something to undermine the apparently inexorable power of objectifying practices. He makes this clear by saying that his point is not that everything is bad, but that everything is dangerous and, he adds, if everything is dangerous, then we always have something to do.

In the "Afterword" to the Dreyfus and Rabinow book (1986), Foucault indicates some forms of opposition to the dangerous effects of power that have been developed over the last few years which exemplify what he has in mind when he talks of rebellion against the objectifying force of power/knowledge apparatuses. He mentions the opposition to the power of men over women; of parents over children; of psychiatry over the mentally ill; of medicine over the population; of administration over the ways people live. The common characteristic that makes these struggles examples of the options open to us is that they do not look for the "chief enemy" but for the immediate enemy; the danger embodied in some particular form of power/knowledge. Their aim is not to discover "truths" hidden by "false" beliefs produced by the power but rather to denounce the detrimental effects of power relations peculiar to a specific social relation. Their main objective is to attack a technique, a form of power which applies itself to immediate everyday life by categorising individuals and imposing upon them truths that they themselves and others have to recognise. To sum up, they are struggles not for or against the individual but against the "government of individualisation". In *Power and Knowledge* (132-33) Foucault says that our participation in such *specific* struggles should take place from a position which can take on a *general* significance. We should operate at the general level of the "regime of truth", that is "the ensemble of rules according to which the true and the false are separated and specific effects of power attached to the true". This regime of truth is essential to the structure and functioning of our society and the struggle is not on behalf of the "truth" but a struggle about the "status" of truth and the role it plays in the structure of our society. For Foucault the essential problem is not that of emancipating truth from the system of power which is a chimera because truth is already power, but that of detaching the power of truth from the forms of social, economical, and cultural hegemony within which it operates at the present time.

Habermas is right to say that Foucault offers us no criteria, nor foundation for the commitments he invites us to make, but we would be wrong to be perturbed by this. In the second and third volumes of the *History of sexuality* Foucault describes a form of life that constructed an ethical system which was not based on criteria, and, more important that did not think of criteria as relevant to the "aesthetic" construction of a good life. Foucault does not tell us this story in order to romanticise the past in an effort to criticise the present, but rather to encourage us to "think otherwise"; to think that demands for foundations, for moral criteria, like Dreyfus's, are the product of our transitory culture. When Foucault (1970: 342) says that, in our day, "it is no longer possible to think other than in the void left by man's disappearance" he does not mean that we must dismiss the problematic of the human subject which is the constant and prevailing preoccupation of his work. What he means is that such problematic must be approached within a new categorical framework where there is no longer a place for those "humanist" categories which have constrained our thinking since Descartes's *cogito*. The void left by man's disappearance, Foucault adds, is "a space in which it is once more possible to think". When we abandon a philosophical position, as Foucaualt does, we create a space in which we can no longer approach philosophical problematics using categories which belong to the superceded position. In the new approach not only the old answers but also the old questions are, as Marx (1978: 148) says "mystifications". In saying that we must think "in the void left by man's disappearance", Foucault says that old problems about human subject must be reformulated within a new conceptual framework where there is no longer a place for categories centred in the hypothetical evidence of an universal, essential self. The arguments of those who accuse Foucault of nihilism are rooted in such "foundationalist" categories. The importance of Foucault's work is that, following Nietzsche's project, it stimulates our capacity to pose questions about what is at stake in trying to think and live without foundations.

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20

Contradictory discourses: An analysis of a male/female conversation

Amanda E. Kottler

Aims

The project had three aims. Firstly, it was my intention to enquire as to whether there was any sex differentiated speech evident in a conversation held between a male and a female. If there was it was my aim to look at what function it served for the speaker. My second aim was to analyse the entire conversation in order to establish (a) What is really going on? (b) What happened between the two participants?and (c) How and why was the outcome reached? Thirdly, it was my intention to establish whether or not the methodology I used proved to be useful.

Background

Some background to the first aim is necessary. Firstly, I worked within an ethnomethodological framework. Secondly, the questions I addressed came about as a consequence of Rosenblum's (1986) review of research carried out thus far into gender and language. Much has looked at "language produced by the sexes" or "sex differentiated speech". Examples are those of dominance (assumed to be the domain of male conversationalists) and interruption (also seen to be a male speech act). An example of language said to be produced by females is that of "telling it slant", a strategy used by women so as to retain some measure of power in a sexist setting (Michell, 1984).

Rosenblum refers to the sets of sex differentiated speech used by males or females as "gender registers". She warns against being influenced by stereotypic expectations, preferring to adopt the idea that "gender registers" are available to, and drawn on by both genders at different times and in different contexts.

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They are therefore used by both genders to greater or lesser extents. She suggests that further research should look into whether or not the "gender registers" are produced intentionally. If so, she proposes that researchers attempt to uncover the function which the registers serve for the users. She posits that their use is related to issues of power, suggesting that when a female register is used it serves to relinquish power and yield the floor to another speaker.

Data and methodology

It was my intention then to carry out Rosenblum's suggestions by analysing a conversation between one male and one female. My data comprised a section on a redundant tape (recorded in 1986) which was part of a differently focused doctoral thesis. Participants were second year psychology students at UCT (both aged 19 years) who had volunteered to take part in a practical course on "Gender Socialisation". They were asked to read, discuss and respond (having reached consensus) to five vignettes, one of which constituted the recorded conversation in this present study. Participants were told that the conversation would be audiotaped but they did not know that it would be used for an analysis of this type.

The vignette read as follows:

Sharon walked along the street on her way home from University. She heard wolf whistles. Turning she saw that these came from a group of men in blue overalls digging on a building site. She felt:

Flattered	Flattered But Confused	Anxious	Angry

Listening to the tape early on I noticed that the female (F) was inconsistent in her responses:

	Line no.
(F) = I'd say I I w'ould feel angry	15

.

.

.

(F) [I wouldn't I WOULDN'T =] 112
.
.
(F) [=SAY ENTI]::rely flattered 115
not entirely flattered 116
so maybe flattered 117
but confused 118
.
.
.
(F): =but generally I'd (.) 169
I'd probably feel 170
flattered? ya? (.) 171
(F): Ok so I'd say flattered 282
but con:f:u::sed 283
.
.
.
(F):['fused OK 292
no I wouldn't I 293
wouldn't say ?angry really 294

This was particularly fascinating and led me to ask the question which influenced my second aim: what is really going on in this conversation? But first, I had to establish what methodology I was going to employ to provide evidence, if there was any, of sex differentiated speech.

My chosen tool was that of Conversation Analysis, a rigorously empirical approach (Levinson, 1983), based on the notion that if the analyst can observe orderliness in conversation then the participants have methodically produced it for each other (Schegloff and Sacks in Heritage and Atkinson, 1984: 2-4). Its object is to isolate regularities or patterns which are common to all conversations, regardless of the speaker's identity or the context in which the discourse has been recorded (Swartz, 1988: 42).

Conversation analysts treat everyday mundane conversation as if it were "anthropologically strange" (Harre and Lamb, 1986: 101). They look at ONE feature of conversation (for example the interruptions I have already mentioned) across a SERIES of recorded conversations.

It is impossible in this short paper to do anything but provide an small example of how I used this methodology. This I will now attempt. First, consider the following extract from the transcript:

(F): = I'd say I I w'ould feel angry 15
(M): (.) you'd feel angry? 16
(F): [beco' 17
(F): =its actually ridiculous 18
because (.) if I really 19
think about it I remember 20
we I I've had this one 21
before but if you really 22
think about it (.) um (.) 23
if it wasn't (.) overalls 24
digging (on) it was a 25
group of business men 26
or something mAY:ybe I I'd 27
feel SLIghtly flattered 28
in some way 29

(M): [um hm 30
(F): cos if if I'd be honest, I 31
know the last group said 32
((eh:: heh::)) I'd be angry 33
you know immediately (.) 34
but - in in that way you 35
sort of almost feel 36
humiliated and - (dem) 37
you know like like as as if 38
if you *regarded as lower 39
class in a way an::* 40

(M): I don't a: there th this 41
is the one thing (well) 42

When in (L16), (M) says: "(.) you'd feel angry?" it appears as if he is asking a question. However, after frequent listening and by applying the work of other conversation analysts it becomes quite clear that he is actually using what is referred to in the literature as a *next turn repair initiator* (NRTI). This is used when there is a hitch which threatens to disrupt the hypothesized organization of the conversation (Levinson, 1983: 339 and 319). In so doing (M) indicates to (F) that he does not agree but he gives her the space to repair, i.e. to change her response to one with which he would agree. As can be seen from line 18, his strategy works; (F) does repair, using an *other initiated,* (by (M)) *self repair* (OI,SR). In line 30 (M), uses "teacher talk": "[um hm" and thereby indicates to (F) that she is on the right track. In so doing he is encouraging her to continue in this vein and he is controlling her talk by directing her speech (Rosenblum, 1986: 167). In another OI,SR (L31) she continues to voice an opinion which is in keeping with that for which (M) has indicated his approval. But, in lines 37-40 (F) begins to express views with which (M) does not agree. This creates a dilemma for (M) because conversations are governed generally by two classical principles - *the co-operative principle* and the *politeness principle* (Swartz, 1988). Conversationalists attempt to ensure that what they say is that which is required and which is expected, given the context. They try not to say too much or too little and attempt to be tactful, generous, modest etc. But, they also go to great lengths to ensure that there is maximum agreement (minimum disagreement) between them (Swartz, 1988: 31-33). This is not to suggest that disagreement does not occur. It does, in a range of guises and strengths, i.e. both weak and strong disagreements are common (Pomerantz, 1984). These notions are connected to the *preference system* in which there are ranked second parts to *adjacency pairs: preferred* or *dispreferred. Dispreferred seconds* typically include some significant delays and prefaces (Levinson, 1983: 307 and 334) in which markers or announcers of dispreferreds such as "Uh", "Well" and "But" are used (Pomerantz, 1984: 72). On the basis of this kind of research, (M's) statement in L41 "I don't a: there th this" is interpreted as a *dispreferred turn.*

This kind of analysis proved to be very useful for my first aim. But for my second it was limited. Whilst it was helpful in describing the features in the conversation and could help me to provide evidence of *turn taking* or *dispreferred turns* and *repair mechanisms* such as those I have already referred to, it was not helpful in telling me what was really happening in this conversation. It did not help me decide how the outcome was reached. For answers to these questions I had to look elsewhere.

I drew on theoretical notions found within the fields of social psychology and semiotics and I also drew on the literature relating to Comprehensive Discourse Analysis, each of which I will now introduce briefly.

Introducing social psychology and semiotics

First, from within the framework of social Psychology I drew from Henriques et al. (1984) who introduce the idea of *multiple discourses,* described as complex systems of regulated differences which are not discretely bounded, nor are their effects predictable. Instead, they are often contradictory. Also introduced is the notion of gendered subjectivity. This is described as a product of the individual's history and the positions he or she has taken up in these multiple discourses. Gendered subjectivity also depends on the psychological investment which the individual might have in taking up a position in the various discourses available at different times and in different contexts. The concept of *psychological investment* to which Henriques et al. (1984) refer is likened to an "emotional commitment" to something, for whatever reason. It is not a rational, nor is it a unitary commitment. Furthermore, it is fundamentally influenced by gender differences (Henriques et al.: 215).[1] As a consequence, individuals have gender specific investments in taking up certain positions in discourses because there is some "pay off" or "reward" for doing so and hence there is a reason for taking it up. This is true even though the take up position might seem irrational and, in terms of other consequences, satisfaction appears contradictory.

Second, from the domain of semiotics I drew on the work of Sless (1986) who demonstrates succinctly that communication is not a "system" and that conversations are not necessarily sites of sharing nor of understanding. These theoretical notions raised questions for me as to the apparent orderliness of conversation referred to by Schegloff and Sacks (in Heritage and Atkinson, 1984: 1).

Finally, I took account of some of the methods used by comprehensive discourse analysts whose main aim is to understand what is really going on in a conversation. They attempt to answer questions about what happened in a particular conversation and how a particular outcome was reached. They do so by looking at the entire transcription rather than at short sequences (as do conversation analysts).

Results and discussion

Applying these theoretical notions I delineated four discourses in a manner similar to that employed by Hollway (1984). The first discourse (D1) (acknowledged by the male participant (M) only at the very end of the conversation) was labeled FLAUNTING GIRLS DO. The following statements are examples of the composition of D1:

	Line No.
(F): you know like like as as if	38
if you °regarded as lower	39
class in a way an::°	40

The implication of this utterance is that only "cheap" and "flaunting girls" are whistled at. In the next four utterances it is suggested that the speaker believes that the whistler's evaluation of the "girl" is correct:

(1) (F): they're regarding you as a 59
sex object and a and a 60
prostitute or 61
something = 62
(2) (F): [= sort of 67
flaunting yourself] 68

(3) (F): [yaah:: but I mean 128
if? you get a girl flaunting 129
herself you immediately = 130

(4) (F): = gonna think that shes:: (.) 132
shes: ahumm (..) a bit: (.) 133
on the on the loose end or 134
whatever 135

From the next two utterances it becomes clear that (F) is of the opinion that something about the "girl" makes men whistle. As a consequence the "girl" being whistled at is seen as "cheap" and "flaunting":

(F): i if someone whistles at you 252
you you think oh hell I've 253
forgotten to put a bra on 254
or something and it looks 255
(.) = 256

(F): [it it it] LOOKS 258
LIKE I'm trying I'm trying 259
to sell myself = 260

I called the second discourse (D2) NICE GIRLS DO TOO. Both (F) and (M) uttered statements indicating their positioning in this discourse at difference stages in the conversation (more so (M) who was seldom positioned within any other discourse). Included in D2 are statements which suggest that wolf whistles are innocent "human" ways of showing appreciation. An example uttered by (M) is found in the following:

(M): [you don't think of the 276
positive things !you're 277
looking !great and - 278
these guys are jus: its a 279
human reaction they 280
sort of just - you know 281

(F) is positioned in D2 when she says:

(F): [because 174
someone's taken notice 175
of you someone's noticed - 176
maybe that you a feminine 177
person or that (.) ?YOU 178
KNOW that maybe you 179
look good that day or - 180
or something 181

Discourse 3 (D3) was labeled PREJUDICE. Suggesting that some of these discourses might be gendered, (F) only ever entered D3 once. She did so only because its introduction by (M) interpellated her into it forcibly and quite unexpectedly - her thoughts elsewhere (Riley, 1987: 38). It is clearly not a comfortable discourse for her to have been forced into. Later she refuses to "hear" any reference to it and therefore never lets herself be positioned in it again.

Utterances suggesting positioning in this discourse are as follows. Referring to the whistlers (M) says:

(M): no::! but I mean (eh::) 92
just because they lower 93
class doesn't mean they 94
can't = 95

(M): = for (exa'ple) aren't you 182
sort of revealing a bit 183
of a prejudice there if 184
if you turn round and 185
see that these guys = 186

Discourse 4 (D4) was labeled WHISTLES ARE INSULTING. (M) was only positioned within this discourse at the end of the conversation. Until then he continually denied its validity. (F) indicated its existence and her positioning in D4 in the following five utterances:

(1) (F): but - in in that way you 35
sort of almost feel 36
humiliated and - (dem) 37

(2) (F): I mean - I'm really being 162
honest I think (probly) 163
most girls would say 164
Oh! no! I - you know 165
terrible! how can you 166
guys do (su'thin) like that = 167

(3) (F): = almost embarrassed in a way 211
(.) if you see w = 212

(4) (F): it just ? depends as well 299
I mean if they - make 300
derogatory comments afer 301
that = 302

(5) (F): [additional BAD 312
comme::: then 313
that I ww:: then I think 314
I'd react differently 315

Having illustrated the four discourses, the following illustrates how, during the conversation (F) slipped in and out of them. It also illuminates the contradictions between these multiple discourses:

(F): = not not quite sure 214
how to react on the whole 215

(F): [you KNOW whether to turn 218
round and smile or to 219
sort of (.) shrug it off 220
or to:: (.) sort of (.) 221
glare or whatever you know 222

It is noteworthy that in lines 214-5 (F) reveals that she is not sure in which discourse to position herself, i.e. which one will yield the greatest pay off, given the circumstances. Her dilemma is evident in the next utterance. I argue that in line 219 she is positioned in D2 (Nice Girls Do Too). By "smiling" the woman being whistled at is acknowledging the "innocence" of the whistle. In line 220, however she is positioned in D1 (Flaunting Girls Do). If she "shrugs it off" she denies any recognition of all the "bad" things which the whistler has attributed to her, i.e. she is suggesting that she is not "flaunting herself". In line 222 she indicates a positioning within D4 (Whistles Are Insulting) in which she acknowledges that she has a right to "glare" and to be angry.

Conclusion

This study had three aims. In concluding, I shall deal first with the enquiry as to what was really going on in the conversation. This aim included a search for the answer to the questions of How and Why the particular outcome was reached. The methodology I employed enabled me to delineate four different and contradictory discourses. This was an important result which facilitated an explanation of what was happening in the conversation. Using this and conversation analysis enabled me to look at the entire conversation in a manner which revealed that the participants constantly repositioned themselves in the various discourses from which they expressed their opinions. It was evident that the repositioning depended to a large extent on the understanding that each had of the circumstances according to their gendered subjectivities. Similarly it depended on the psychological investment involved and the likely pay off.

The analysis revealed also that the way in which (M) and (F) introduced topics, collaboratively ratified them, competed over and closed them down, followed the rules governing the structure of conversation and its overall organization. They also went to great lengths to ensure that there was maximum agreement. It is possible however that this might have occurred because of the task which was set for the participants (i.e. consensus was called for) or that it was for the benefit of the audience. This design problem is addressed below.

A remarkable finding of this study relates to the evidence that there is orderliness in this conversation. This is unexpected because of Henriques et al.'s (1984) ideas of multiple and contradictory discourses, their notions of differently gendered subjectivities and of psychological investments. It is also unexpected because of Sless's (1986) convincing argument that communication lacks both sharing and understanding. This finding warrants further investigation and explanation from within this ethnomethodological framework.

The second aim of the study was to enquire into whether or not there was present in the conversation sex differentiated speech and if so, what function it served for the speaker. There was some evidence of the use of such speech. (M) used teacher talk and dispreferred turns more than did (F). (F) used a strategy in which, if there was to be no pay off, she did not "hear" much of what (M) said. This was not a tactic used by (M). Both of them "told it slant" (I suggest that the response "flattered but confused" can only be interpreted as such). (M) did not dominate nor did he control the conversation. Both participants did so at different times. It is not at all apparent that (M) forced (F) to change her response from "Angry" to "Flattered but confused" (which stereotypic thinking might lead us to expect). Similarly each interrupted the other equally often.

In spite of these differences, it is difficult to determine what function, if any, the various strategies served for the participants. I conclude therefore that for purposes of this particular aim, the design of the study was not satisfactory. Applying conversation analysis to only one conversation and searching for more than one phenomenon proved problematic. This leads me on to the third and final aim of the study which was to comment on whether the methodology used was successful.

The results and a commentary on what was going on in the conversation would have been impossible without having applied both conversation analysis and comprehensive discourse analysis to the conversation. From this perspective then, the methodology was successful. However, as I have said it was not suited to the second aim - to establish whether or not there was sex differentiated speech present. To this end, conversation analysis on its own would have been more suitable (if applied to many more excerpts, i.e a series of conversations in which, for example, only interruptions were considered). Another problem relates to the notion of the audience - a confounding variable which has to be taken into account in this discussion. As a consequence conclusions about sex differentiated speech can only be tentative.

Because of the success of the first aim, I argue that this study has demonstrated powerfully that conversation analysis and comprehensive discourse analysis are certainly not incompatible. I suggest further that whilst they are important methods on their own, if combined and applied to ethnomethodological research, they would be crucial; this would help to uncover that which is not expected but which occurs and is important to know about. This study has demonstrated that such discoveries are both informative and extremely useful for enquiry certainly within social psychology and presumably a host of other disciplines in the social sciences.

Notes

1. This is not to suggest that there are no differences within gender groupings. See Barrett (1987) for an excellent discussion on this issue.

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21

Discourses on childhood sexual abuse

Ann Levett

1. Background: Discourse and cultural hegemony - the production and reproduction of tacit knowledge

According to Kenneth Gergen (1985) social constructionism is concerned with clarifying the processes through which people come to describe, explain or interpret the world in which they live. Like Foucault's work on the development of epistemology (Foucault,1972), constructionism is critical of positivist-empirical scientific endeavours and offers the psychologist alternative routes to understanding psychological phenomena through an examination of language usage, contextualised within particular sociocultural and historical moments. Contextualised constructionism is a realist rather than a relativist approach in that it argues that social knowledge is negotiated within cultural contexts and that our theories need to incorporate the specific sociocultural origins of the constructions which prevail (Restivo, 1988).

In everyday life it is taken for granted that there is commonality in perception of social reality and its significance, and a particular range of categories and accounts comes to be viewed as natural. Berger and Luckmann (1971) and Garfinkel (1967) among others have discussed the role of talk in the maintenance and modification of shared meaning, which results in subjective accounts being accepted as commonsense or tacit knowledge. Many scientists (not only social scientists) have put the view that we learn to "read" experience in terms of existing interpretative repertoires (Heritage, 1984). Increasingly, there is argument that the distinctions currently made between tacit knowledge and the supposedly more rigorous realities of science are false ones (O'Neill, 1986; Keesing, 1987; Restivo, 1988). These contentions need not be understood as anti-science but as essentially reflexive developments, to guide thoughtful re-appraisal of entrenched ideas in everyday practices. Moscovici (1984) ar-

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gued for the study of the processes involved in the maintenance and transmission of social representations and several such studies appear in Farr and Moscovici (1984). Various psychologists have published analyses of discourse - for example, Gilbert and Mulkay (1980) on the talk of scientists about their research practices, Wetherell (1982) on femininity, Hollway (1984) on male and female sexuality, and Potter and Reicher (1987) analysed accounts of race riots in the UK; Potter andWetherell (1987) have published an introductory text to the use of discourse analysis in social psychology.

Recent work argues that explanations and transformations of personal and social experience are provided in psychology in ways which serve to regulate experience through particular explanations and classifications (Henriques, Hollway, Urwin, Venn and Walkerdine, 1984). Thus hegemonic or dominant discourses are discernable. Foucault's writings on the lattices of power which permeate the production and reproduction of individual subjectivities, of social groups and identities and of broader institutional power structures were precursory to these ideas. Writings and verbal statements, as well as codes of conduct and institutionalised regulations - all viewed as discourse - are generated in an ongoing social context. They shape available interpretative repertoires for human experience, particularly experiences which are valorised and emotionally laden.

Social science which seeks regularities through positivistic research and low-level interpretation, finds these in noncontroversial sets of ideas about situations and experiences. These generally are the dominant interpretative repertoires. However, closer and effortful examination throws up contradictory or confusing subdominant discourses as well. These tend to be overlooked, discounted or trivialised by researchers although human subjects, being agentic (Giddens, 1983; Shotter, 1984), develop alternative repertoires or metacommentaries while participating in dominant discourses shaped by hegemonic processes (Karp, 1987). Some "objects" are overdetermined by dominant discourses in certain respects (e.g. mothers, wives, children) through overlap and reverberation between fields of discourse (Minson, 1980), but there is no single, simple dominant discourse in any social context. Thus interpretative repertoires (and tacit knowledge) may be modified, from one context to another or gradually over time, through discursive contradictions and through agentic shifts. Along with the dominant discourses about sexual abuse to be found among the lay public, echoing and re-echoing the professional literature and media, there are also muted or less obvious anomalous themes. In Foucault's terms, this muted resistance is always present (the "counterstroke" of relations of power) and it is here that agentic aspects may be identifiable (Gordon, 1980).

The importance of discourse analysis is that it has the potential to enable people to understand the nature and the effects of their everyday practices, the better to recognize gaps and contradictions in dominant discourses and thus to modify them. Speaking from a subjectivity which has been formed within dominant discourses it is, of course, extremely difficult to discern the muted or contradictory themes - but it is crucial to attempt to do so.

Professional discourse is self-perpetuating and hegemonic in that it is generated by a socially powerful group, filters through the media (whose goals favour a naive positivism), through formal education and word of mouth, to perpetuate hegemonic discourse among the lay public, where it is echoed verbally and in performance. Those who have the right to make judgements about the effects of certain events have power, and their assessments consolidate this power. Those who are assessed, named, and classified, (and who assess, name and classify themselves) are consolidated in positions of relative powerlessness in certain respects. In the context of childhood sexual abuse, part of this process involves the shaping of individual subjectivities in particular ways: the production and reproduction of victims (women and children) who need protection, and of villians who are individually pathologised and who require punishment or treatment. The social sources of the dynamics at play are rendered invisible in this account.

In this interpretative study, some dominant discourses surrounding notions of psychological trauma associated with childhood sexual abuse are displayed and criticised. The term discourse will be used here to describe spoken and written statements which constitute particular constructions of events and emotional experience, although the term also encompasses legislation, intervention practices, professional instruction, etc. Following a brief synopsis of current empirical-clinical views, an analysis of lay discourse (verbal statements about childhood sexual abuse collected from middle class women) will be presented. The most common repeating themes found in lay discourse are similar to the interpretative repertoires present among professional mental health writings. Some fragmentary contrapuntal themes which have not been caught in the nets of empirical studies will also be identified, and interpreted as potential sites of resistance. These do not fit comfortably within dominant discourses, nor with the notion that experiences of sexual abuse and molestation are uncommon for girls and women.

The sources of comparison for the material to be subjected to analyses are recent literature reviews of empirical-clinical research (Browne and Finkelhor, 1986; Levett, 1989a), and unprepared spoken material collected from 93 women who talked about childhood sexual abuse case vignettes, transcribed from tape recorded small group discussions (Levett, 1989a) and subjected to several analyses, only a fragment of which will be presented here. Of these women, 44% identified themselves as having been sexually abused or molested during childhood (Levett, 1989b).

2. Dominant interpretative repertoires in empirical-clinical studies of child sexual abuse effects

Experience of childhood sexual abuse, broadly conceived, is common among women in western society. Recent prevalence studies suggest that 40 to 50% of women give accounts of such experience (Russell, 1984; Levett, 1989b). In spite of a growing literature on human resilience in the face of personal problems and adversities (Taylor, 1983; Rutter, 1985; Swartz and Levett, 1989), ideas of inevitable emotional scarring or damage following such experience have become widely accepted in the past 10 years. The discourse on psychological trauma is fraught with confusion - particularly where it concerns childhood experiences. Although recently some have commented that it is doubtful whether current problems can ever be argued to relate directly to early sexual abuse (Jehu and Gazan, 1983; Fromuth, 1986; Conte and Schuerman, 1987), most research has involved a search for symptoms among identified "victims" - those who disclose such histories. Any of a range of behaviours viewed as deviant, relative to a range of socially mediated norms, are identified as effects. The predominant ways in which trauma has been conceptualised and studied are briefly outlined here as the dominant discourses on trauma.

The search for signs and symptoms

This is the dominant discourse: a range of current signs, ills or complaints are related to the experience, the particular signs being ones agreed by convention. An enormous range of signs and symptoms has been identified as evidence of recent or present sexual abuse, or of long term effects. This is something like the production of a laundry list (Mestrovic, 1985). The reading of symptoms has much to do with where and how an individual is encountered, and what is involved is a socially constructed code rather than a set of simple, observable facts, independent of socio-historical forces. The code is validated by journal publication, textbooks and professional socialisation. Two kinds of research are most likely to be regarded as successful: the kind that substantiates previous findings and the kind that addresses issues which are identified as relevant to particular problem areas, i.e. pressures to right wrongs, to restore some mythical state of health, and to tuck in all the loose ends which signify evidence of chaos or conflict in a particular social context. Judgements of handicap and disability are involved. Many have argued that what seems to be a humane enterprise can also be interpreted as a subtle form of control by powerful, if impersonal, interest groups (Corrigan and Leonard, 1981).

Post-traumatic stress disorder (PTSD)

Recently there has been a remarkable increase in attention to "stress" and to what are regarded as the psychosocial consequences of stressful events (Figley, 1985). A particular set of signs and symptoms has been demarcated as a specific diagnostic category called PTSD, although its precise aetiology and pathogenesis are unclear - as invisible as the King's clothes in Hans Christian Anderson's story (Trimble, 1985). A semblance of order is offered through a diagnostic label and increasing numbers of clinicians have seized on it, as though self-explanatory. This has begun to be applied in cases of childhood sexual abuse too (Gelinas, 1983; Donaldson and Gardner, 1985; Green, 1983) - even though the major criterion for applying the diagnosis is that an extraordinary event has occurred, one "outside the range of usual human experience" (APA, 1980). The use of the diagnosis obscures the social causes of events, renders the discursive aspects of psychopathological labelling invisible, and indefinitely postpones attempts to deal with the issues involved.

Deviance as a consequence

The singling out of particular behaviours or developmental pathways, judged as deviant relative to some mythical set of norms, was much more obvious in discourses of traumatic effects in the 1960s and 1970s, but continues in subtle forms today. This approach takes no account of the range of cultural conditions and sociohistorical forces involved - in behaviours such as prostitution, delinquency, substance abuse, etc., in lifestyle choices such as lesbianism or remaining single, or in the assumptions involved in western models of child development (Levett, 1989c).

The assessment of children's behaviours and emotional state, or of women's low self-esteem, problems with relationships, and preoccupations with notions of sexual dysfunction, are all based in assumptions of health or normality. Yet the greatest agreement about long term effects of childhood sexual abuse is to be found in these central areas of women's lives - a consensus which takes no account of the generalised consequences for female subjectivities simply of *being* girls or women in a particular social formation.

Professional discourses

A representative example of the kind of study of concurrent effects reported in the literature on child sexual abuse is to be found in Adams-Tucker (1982). The array of problems was grouped into 6 clusters, ranked in order of seriousness: (1) self-destructive behaviours, withdrawal and hallucinations, (2) aggression, sex-related complaints and running away, (3) problems concerning school and oppositional difficulties with parents, siblings or peers, (4) anxiety, (5) psychosomatic complaints, and (6) sleep-related complaints.

The main signs and symptoms which have been identified as long term traumatic effects of childhood sexual abuse (Steele and Alexander, 1981; Browne and Finkelhor, 1986) are poor self-esteem, self destructive behaviours (widely conceived), problems with intimate relationships, mistrust of men, sexual dysfunction, depression, and a range of behaviours such as promiscuity, illegitimate births, truanting, substance abuse, prostitution, lesbian preference.

Factorial models

Efforts to formalise the varying outcome of childhood experience of sexual abuse in its different forms have been reported; these involve attempts to weight the relative significance of a never-expanding number of factors in a range of ways, making use of continua or dimensions (e.g. age, degree of relationships, type of sexual abuse, recurrences, degrees of violence, etc). Finkelhor and Browne (1985) evolved four dimensions to traumatic effects (sexualisation, betrayal, powerlessness and stigmatization) and argue that the effects vary relative to cognitive development and understanding of the experience. They say finally that children will "...emerge from their experiences with inappropriate repertoires of sexual behaviour" and "confusions and misconceptions about their sexual self-concepts" and "unusual emotional associations to sexual activities". These are loaded statements and do not really add much to the picture previously available. The most interesting aspect of Finkelhor and Browne's paper is the significance they give to stigmatisation since almost nothing has been done to research this aspect of childhood sexual abuse. However, the judgements cited involve implicit assumptions of appropriate sexual behaviour for children, correct and incorrect conceptions of self, and a conventionally simplistic view of childhood development and emotions. It is not recognised that incorporations of notions of betrayal, understanding relative power and powerlessness, sexual and gender related exploration, are part of the development of human subjectivity at all ages, in any human group.

3. Analysis of lay discourse on child sexual abuse among women: Dominant and anomalous themes

Some 1200 lay discursive statements were subjected to a series of analyses which grouped and regrouped the statements in various ways, in a number of repeated siftings of the field of statements. For this particular study, statements containing words (especially adjectives) or terms with an obvious relationship to the symptomatology and interpretations used by mental health professionals (psychologists, psychiatrists, social workers, doctors) were grouped together in order to examine the limits of dominant discourses (Table 1). All residual

348

statements were also examined. The statements were grouped around emergent, conceptually meaningful themes, making use of borderline or contradictory statements to highlight anomalous discourse. It is important to stress that there is little similarity between the analysis of discourses and content analysis (Potter and Wetherell, 1987).

Concurrent traumatic effects

The statements of Negative consequences for children involved in situations of non-violent sexual abuse comprised 70% of statements concerning concurrent effects (590 of 844 statements). Most (546) were judgements very similar to those found in the professional literature. A few typical examples of these statements are:

She'd get withdrawn because I would get withdrawn.

She might be quite attention seeking.

She'd become wary, evasive.

Very mistrustful.

She'd be very nervy, anxious.

She might want to bury herself in other activities to forget.

Always going to her teachers: I've got a tummy ache, or I've got a headache.

She might have problems at school.

The remaining 30% of statements analysed (231) were ones which did not involve Negative effects but often did involve judgements of relative severity, similar to the ameliorating and exacerbating factors often discussed in the professional literature. Some typical examples of these statements are:

She was very young and .. I mean, he does not seem to have molested her very severely.

It's only one incident you know, it's not a gradual process that's building up.

He was only touching her - not violently, not for long.

Long term effects

A total of 231 statements were extracted concerning long term effects; 75% of these described Negative traumatic effects. Typical examples of these statements are:

She'll get increasingly negative about her self image.

She could take a very negative view of her body.

They block off their sexual feelings.

They don't want to change: they don't develop breasts and that kind of thing. Anorexia and such.

It could ... make her sort of overtly sexual, more promiscuous.

The girl is usually scarred for life.
[She'll have] problems with relationships.

Here again, many statements, while not involving judgements of inevitable traumatic effects, did incorporate the kinds of comment which would belikely to be encountered in the Professional literature (e.g. that if there were no traumatic consequences these could be explained by particular case specific factors). Typical examples of these statements are:

I think because it wasn't actually her father, maybe the effects wouldn't be so great.
Because she's so young, I don't think she actually realized what was going on.
It's actually worse with someone you know.

Discourse analysis revealed that statements concerned signs and symptoms of traumatic effects, or themes of trauma similar to professional interpretations in a range of ways (see Table 1). Some examples of these statements follow:
Withdrawn, pensive, quiet.
[She'd have] difficulty with her concentration.
A friend of mind became a very manipulative type of person.
A general wariness of men.
She would feel dirty, maybe, strange.
Pervertedly interested in phallic symbols or something.
She'll like completely repress it.

Also similar to what is found in the professional discourse, there was a gap evident in the talk about child sexual abuse: there is a great deal more about the effects of sexual abuse on the child than attempts to explain why men do these things. Implicitly, it is as though dominant discourses about male sexual drives (Hollway, 1984) suffice as an "obvious" explanation.

Anomalous discourses

These fall into three clusters: (1) "naturalisation" of child sexual abuse, (2) agentic strategies: laughter, secrets and silence; (3) interpellation of the subject as female into the discourse of sexual abuse.

(1) *"Naturalisation"* of child sexual abuse is found more commonly in the lay discourse than in the professional literature. It emerges here in two forms. One form occurs in a cluster of lay statements which simply express the belief that there would be no traumatic effects, short or long-term, in terms of signs

and symptoms. This is a view which is rarely and cautiously presented in the professional literature as an unusual situation, one difficult to explain, or as latent effects (which will emerge at some point, following a "triggering" event). In the other form, there are statements which hold that there would be no traumatic effects because there is no reason for the child to think that she would be affected. Some examples of such statements are:

She most probably thought it might even be natural.
She probably won't think about it.
I think she would just reconcile herself to what had happened.
If you didn't feel it was bad at the time it'll affect you less.

(2) *Agentic strategies:* Active attempts by the child to understand or manage the situation, cognitively or physically, were mentioned (sometimes in association with personal accounts of similar experiences) and were interpreted as ways to subvert the child's sense of powerlessness. Some examples follow:

When she gets older she might just laugh about it.
They were just playing: everybody played mommy and daddy.
It was like sort of, a secret friend, probably.

The reluctance of children (and of adults) to disclose early sexual experience is often commented. This is related to traumatic effects in the professional literature (Browne and Finkelhor, 1986). However, part of the value of secrets is as a way to achieve or maintain a preferred position, one of relative power or non-marginality, and as a way to deal with stigma, thus secrecy and silence must be understood as agentic strategies even though they may also consolidate aspects of oppression or exploitation. Discourses of sexuality in western societies involve powerful discourses of silence in a range of respects (Foucault, 1979) and decisions to disclose certain experiences involve conventions of confession or therapy, and generally of interpellating oneself within discourses of victimology, deviance or pathology.

(3) *Interpellation of female subjectivity:* From a young age girls are greeted, commented on, complimented or criticised as females and thus of potentially sexual interest to males (Haug, 1987; Riley, 1987). This is an important aspect of the development of female gendered subjectivity. Each instance of sexual abuse or molestation, occurring or in its avoidance, is a moment of interpellation as female into the social order. This complex idea is reflected in certain of the statements analysed, as follows:

Now she would become aware of the difference between guys and girls.

So, she is different from the rest of the boys.

The day that happened to me, suddenly I realised that I was a girl. I mean, just realising I couldn't go around with the boys any more.

Concluding comments

In conclusion, research on the effects of child sexual abuse exemplifies the kind of empiricism which produces and perpetuates particular forms of knowledge, as social products (Young, 1980): research and tacit knowledge mirror one another and serve to maintain existing social structures. A fundamental issue which is not addressed in contemporary discourses on the traumatic effects of sexual abuse is the powerful effect of sets of social beliefs associated with this experience. One might draw useful parallels with anthropological studies of pollution (Douglas, 1966) or bewitchment (Favret-Saada, 1980). Although this has not been done here, as a step in that direction an analysis of lay discourse on the effects of childhood sexual abuse is presented here.

Despite common ideas about the significance of problematic events in childhood (family conflict, losses and separations, particular childrearing practices, or abuse), these ideas have not been supported in rigorous positivistic research. What is clear is that attributional beliefs about traumatic effects and negative consequences of certain events have a great deal to do with the experience of these consequences. In other words, one of the most important aspects of the discourse on traumatic effects of sexual abuse is the way that in dominant discourses such experience in itself signifies damage and may well introduce ideas of difference (or stigmatic effects) among those with experience of child sexual abuse. Stigmatic effects and discourses of resistance to such notions, such as are presented here, need further exploration.

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TABLE 1

Analysis of discourses: Emergent themes on childhood sexual abuse effects

	No. of statements	%
Signs and symptoms	173	19.2
Psychological interpretations (e.g. psychoanalytic = neurotic)	125	13.8
Effects on		
Psychological development	167	18.5
Relationships	120	13.3
Sexuality	78	8.6
Self-esteem, subjectivity	56	6.2
Stigmatic effects and silence	60	6.6
OTHER EFFECTS AND CONSEQUENCES		
Rules, warnings and restrictions	62	6.9
Media allusions	19	2.1
Anomalous statements	40	4.4
TOTAL	900	100

The phenomenological and other qualitative views

22

Research and reality: How clinical theory and practice are actually developed - Case study method in cognitive behaviour therapy

David J.A. Edwards

The nature of research in cognitive-behaviour therapy

Contemporary cognitive behavioural psychotherapy offers the therapist an integration of the practical effectiveness of traditional behavioural techniques with the humanistic values of the existential therapies and the historical developmental perspective of the psychodymanic therapies. It provides an integrative framework which allows the incorporation of a range of theoretical perspectives and technical approaches (see, for example, Arnkoff, 1981; Chambless, 1985; Edwards, 1989 and in press (a) & (b); Goldfried, 1988). However, its ideological allegiance has been to the natural scientific tradition in which the main focus of research procedure is on quantitative methodologies. Despite this, theory and practice have largely advanced by the accumulation of clinical wisdom based on extensive experience with individual cases.

This is reflected in much of the contemporary literature, for example, in the two major cognitive therapy treatment manuals from the Beck group, on depression (Beck, Rush, Shaw & Emery, 1979) and anxiety (Beck & Emery, 1984) as well as a contemporary behavior therapy handbook (Hersen & Bellack, 1985). These provide detailed clinical guidelines for case assessment, conceptualisation and the formulation of treatment strategies which essentially reflect the experience of many clinicians over a number of years working with case material in seminars, workshops and at conferences to develop and test the ideas they contain.

The theory and methods are richly illustrated with case material. However the links between theoretical and technical development and the case material

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are not often made explicit. Nor is the status of the use of case material as research data formally recognised. As a result, its value as a contribution towards research is often underplayed. Yet case study research is a long established, if ignored methodology (Bromley, 1986; Mitchell, 1983; Yin, 1984). In this paper its fundamental principles, as well as the reasons why it has been neglected will be examined.

What is research? Scientist versus practitioner

Research in psychotherapy has long been a subject fraught with ideological conflict. Historically, an inappropriate focus on a quantitative, nomothetic methodology modelled on physics resulted in the neglect of the naturalistic, ecological, case-study approach (Kiesler, 1981: 212-3). Even in Sociology where case study research had an established reputation in the first half of the century, there was, in the 1950's, a decisive shift towards quantitative methods and a complete decline in respect for and use of case studies, linked to advances in computer technology (Mitchell, 1983).

In clinical psychology it is naturalistic idiographic methods that have constituted an influential, if informal, sector of clinical research through which the theory and practice suited to the particular problems of clinical work have actually been developed. Strupp (1981: 216) rightly observes that what we know about psychotherapy "has come from astute and creative clinical observations". By contrast the elaborate technology of formal experimental design and statistics has been of relatively little practical help to clinicians, with the paradoxical result that "clinical research has little or no influence on clinical practice" (Barlow, 1981: 147). Because of an inappropriate emphasis on quantitative experimental designs and the resulting eclipse of other research methods, the method of quantitative group comparison came to be perceived as the only form of research (Hayes, 1981: 194).

Journal policies which favoured brief quantitative studies, together with the pressure on researchers to publish in quantity encouraged a proliferation of papers on overspecialised topics of little practical relevance in the clinical setting, most of which are read by very few. 70% of members of the American Psychological Association subscribe to no APA journals, and 63% of British Psychological Society members to no BPS journals. The 1981-3 *Psychological Abstracts* has only 35 entries on a practical issue of widespread concern, "step-parents" and "step-children" while there were 330 entries on the far less common phenomenon of "autism" and 1800 on "statistics" (Standing & McKelvie, 1986).

Barlow (1981) observed that the split between scientist and practitioner in clinical psychology was noted as long ago as 1961 and that clinical psychologists answering surveys recommended that less time should be devoted to teaching research methodology during training. I suspect that the same attitude would be found in present day South Africa. This issue is particularly pertinent in view of the fact that the Human Sciences Research Council requires that 25% of the marks for a directed masters course in clinical Psychology should be given for research or courses on research method as a condition for giving bursaries to students.

Clinical students frequently find themselves alienated from the concept of research by a training in methods that are artificial and unproductive, performing research studies which fail to enhance the quality of their clinical work, within a paradigm which rules out of order the questions which really interest them (Hayes, 1981; Kiesler, 1981).

The psychotherapist's special access to quality research data

A more realistic perspective is the practitioner-as-scientist model within which the conduct of psychotherapy is recognised as a fundamental research enterprise. The first aspect of this model is that therapists have privileged access to data relevant to a range of questions related not only to emotional and behavioural disorders but to fundamental existential and human questions. Within the context of the trusting relationship that is essential to psychotherapy, clients verbalise thoughts and feelings and explore issues which they do not usually broach in other contexts. In the daily course of their work, therefore, therapists are exposed to the depth of a person's experience that is not easily accessed by research methods using questionnaires or one-off structured interviews.

In order to deepen their access to their clients' worlds, therapists develop basic skills of listening, empathic responding and searching questioning. A high degree of sensitivity, commitment and personal integrity are required if a person is to explore issues of deep emotional significance over a long series of sessions as may occur in therapy (Bromley, 1986: 24). The more therapists embody these skills the more access they have to the depths of their clients' unspoken feelings, attitudes and motivations. It is on this basis that Kvale (1986: 155) claims that psychoanalysis is "*the* qualitative research method" that has yielded substantial advances in our psychological knowledge. The skills required to achieve this, while essential to clinical practice are therefore, also, fundamentally, research skills.

The professional context as a structured research setting

The second aspect of the practitioner-as-scientist model is that the professional context of psychotherapy, with its attendant work of case conceptualisation, supervision and peer discussion through seminars, conferences and publications, provides an infrastructure for systematically advancing knowledge. Unfortunately, historically, the safeguards that are the hallmark of careful research have often been neglected. Strupp has succinctly summarised the methodological flaws which vitiated psychoanalysis as a research enterprise in a catalogue of sins that is a challenge to any training program in psychotherapy or research:

> total commitment to a single theoretical and technical model; enshrinement of theoretical formulations as dogma; imperviousness to research progress in neighbouring fields; rigid indoctrination of trainees; failure to inspire students to become critical and independent thinkers; failure to teach students abiding respect for empirical data and rival hypotheses; insufficient attention to clinical realities ... (1981: 217-218).

These problems are best overcome by attending to the issues Strupp raises in teaching the proper use of case study method, rather than by refuge in inappropriate methodologies.

Psychotherapy as applied science

Thirdly, the therapist is an applied scientist who, like a field geologist, or an ecologist, uses the framework of existing knowledge about a particular area in order to systematically analyse and solve problems. Every case is new and unique and requires the careful observation and conceptualisation that is typical of a scientific exploration of a new phenomenon (Kiesler, 1981: 214). Decisions regarding management or intervention stategies are made by applying already existing knowledge or by extending it in the same way that is done when any problem is made the focus of formal research (Hayes, 1981: 194).

The principles of case study method, as set out in the recent literature, (Bromley, 1986; Mitchell, 1983; Yin, 1984) provide a framework which readily incorporates the practitioner-as-scientist model and overcomes the scientist-practitioner split in clinical psychology. This literature formally describes methods for using case material for developing theory, and for establishing the validity and reliability of conceptualisations and conclusions.

Principles of case study research methodology

A case study requires the accurate and careful description of the phenomenon being investigated, and the use of the description to generate a conceptualisation which embodies general principles (Mitchell, 1983). This is done in, or as close as possible to, the real-life context in which the phenomenon occurs and evidence relating to the phenomenon is obtained by a range of appropriate means (Yin, 1984: 23). The resultant conceptualisation opens up the essential qualities of the case being investigated and is tested, refined and extended by a series of subsequent case studies. Bromley (1986) likens the process to the building up of case-law in jurisprudence. Once the case-law is well developed the majority of cases can be handled without extending it further, but others will require its further refinement and extension.

Case study research does not use statistical inference. Validity is established by a logical process termed "analytic generalisation" by Yin (1964: 39) or "analytical induction" by Mitchell (1983: 200). It is not assumed that a case being studied is typical or representative, but that the key aspects of theory implicit or explicit in the presentation of the case material will be of value in conceptualising other cases.

Case studies serve a number of different goals, as will be clear from the studies analysed later in this paper. First, they may be illustrative-didactic. Gluckman refers to the use of case vignette as an "apt illustration" (Mitchell, 1983). The cognitive-behavioural literature includes a continuum of illustrative-didactic case material ranging from short vignettes, to extended case descriptions chosen to render theoretical principles quickly accessible to the reader. Such case material is not only illustrative but provides evidence for the generality and validity of case law. The writer is in effect claiming, "Here are the principles of case law for this type of case and here is an example of an observation which substantiates these principles". In the laboratory, a good experiment is also an elegant demonstration. It not only rules out certain hypotheses, but economically illustrates the theoretical framework that it supports. Illustrative case material can constitute a similar elegant demonstration and is no less valid because it is based on material from a single case.

Three other goals of case studies are referred to by Ekstein (1975) and Mohanty (Giorgi, 1986, p. 10). First there is the goal of achieving an accurate description (Ekstein's "configurative-idiographic" and Mohanty's "phenomenological descriptive" approach); second there is that of developing a useful conceptualisation (Ekstein's "heuristic" and Mohanty's "theoretical framework construction" and third there is that of critically testing specific aspects of already developed theory (Ekstein's "crucial case study" and Mohanty's "hypothetical-confirmatory-disconfirmatory" approach).

Phenomenological research is largely a type of descriptive case study which aims to incorporate little conceptualisation beyond the givens in the data (cf. Giorgi, 1986). This is appropriate where the phenomenon in question is unusual and little understood. However, the cognitive behavioural literature largely focusses on common and fairly well defined clinical problems so that most case study work is concerned with constructing theoretical frameworks, or providing critical tests of theory.

Establishing validity in case study research

In order to enhance the validity of the conclusions enshrined in the theory or case law, careful procedures need to be established for collecting and evaluating evidence. Links in the argument whereby the conceptualisation is built up relating one aspect to another or linking the theory to the data need to be logically argued and internally coherent. Alternative explanations and theories need to be canvassed, which should be specific enough to be testable, and cases should be sought which enable competing theories to be evaluated against each other ("test cases") (Bromley, 1986: 26).

For Bromley (1986: ix-xii) the case study is "the bedrock of scientific investigation" so that to learn to execute a case study is to receive a fundamental training in a scientific method which is as rigorous and informative as experimental or survey research (Bromley, 1986: 286). On the other hand, both Kazdin (1981) and Kiesler (1981) are at least partially seduced by the ideological principle that only formal experimentation can yield scientifically valid results. Kazdin, despite his recognition that "The case study has played a central role in clinical psychology" (p.183), also argues that there is a continuum from case studies to experiments, with scientific validity being stronger at the experimental pole (p.185). Kiesler takes the same view, referring to pre-experimental, quasi-experimental and experimental studies.

There are a number of reasons why experimental methodology is not generally superior to or more valid than a case-study approach. First of all, experiments are usually formal tests of hypotheses. Meaningful hypotheses can only be derived after conceptualisation of a phenomenon. This requires a foundation of careful descriptive work which can only be done by in depth examination of individual cases.

Secondly, case study method provides a solid foundation for establishing external validity (which concerns the extent to which conclusions drawn from a study can be generalised outside of the case or sample studied), since clinical case law is subject to repeated tests against new cases. Unfortunately, much of this testing is not reported in formal research writing although its results are shared and discussed in case conferences. There is a need for the research literature to reflect this process more systematically.

Thirdly, internal validity can also be established by means of case studies. This refers to the validity of inferences about causal relationships. It only applies, therefore, where inferences are made about causality and is not in question when research is descriptive (Yin, 1984). Kazdin (1981) has shown how the internal validity of conclusions about the effectiveness of a therapeutic intervention can be established from case studies. Threats to the validity of conclusions about the effectiveness of a therapeutic intervention include the possibility of: effect of external events in the client's life (depression and loneliness end due to forming new relationship), spontaneous psychological change (completion of mourning, spontaneous acquisition of coping strategies), habituation effects due to repeated exposure to the means used to assess outcome, changes in criteria for assessment, or statistical regression. These threats can be largely eliminated if case studies employ: (1) objective means of data collection, (2) repeated assessments using the same method or instrument (3) multiple cases. There is also strong evidence for efficacy where effects are sudden and marked ("slam bang" effects) and where the remission coincides with therapy when the symptom is long-standing.

Finally, experimental methodology does not necessarily yield more valid conclusions than qualitative methodology, because of the inevitable trade-off between internal and external validity (Kiesler, 1981). Threats to internal validity are met by controlling a large number of independent variables. Unfortunately the resultant context may be so artificial that conclusions drawn do not apply to real life situations. Thus internal validity has been obtained at the expense of external validity. Conversely, investigations of realistic life situations may have difficulty in establishing internal validity because variables which could be causal factors are uncontrolled or even unrecognised. Here, internal validity has been sacrificed to achieve external validity. The case study method offers a solution to this dilemma. It preserves external validity by investigating persons or events in or as close as possible to their natural contexts (Yin, 1984: 23 and it employs logical inference to enhance internal validity. Two examples of systematic ways in which this can be done are the use of multiple cases to build up case law as already described and the observation of regular patterning in events as in time-series analysis (Hayes, 1981; Yin, 1984).

Case study research in the cognitive-behavioural literature

The cognitive-behavioural literature is rich in case material. There are a number of books which are largely collections of case-studies (e.g Emery, Hollon & Bedrosian, 1981; Eysenck, 1976; Hersen & Last, 1985; Lazarus, 1985), and the clinical manuals from the Beck group (Beck, Rush, Shaw & Emery, 1979; Beck & Emery, 1984) are typical in being full of case vignettes and longer case examples. The literature also includes many books or chapters in which case material forms the basis for describing or explaining the approach, or for reaching conclusions about the value of specific theories, strategies and tactics. Some different uses of case material will be critically examined in the following sections.

Single case presentations

A number of case studies serve as didactic illustrations of a framework of case-law that has been developed, furnish evidence for its validity, and may lead to the extension of that case law. For example, Chor, Mercier & Halper (1988) described the cognitive therapy of a case of bipolar disorder. They showed how the client's treatment during her depressed phases fitted the principles of cognitive therapy for depression, but extended the existing technical approach to deal with her manic phases. They incorrectly describe it as a descriptive case study. It does not offer the in depth phenomenological description described by Giorgi (1986) but rather confirms and develops an existing conceptual framework. The study fulfils many of Kazdin's (1981) conditions for establishing internal validity, and the authors were able to argue cogently for the effectiveness of the interventions since, for the first time in 24 years the client experienced a two month period of level mood.

Friedberg's (1987) description of a case of psychogenic vomiting develops case law by providing a thorough cognitive and behavioural analysis for an unusual disorder. Since the problem was long-standing and had not responded to previous treatments (two years of psychoanalytic therapy and 18 months of thorazine), and since reliable records of vomiting frequency were available over a number of years including 3 years following the intervention, it provides good evidence for internal validity. The report describes the rationale for the treatment interventions and the manner in which they could be seen to alter the client's beliefs, emotions and behaviour in a manner which could be logically linked to the cessation in the vomiting.

The author concludes this very good report unnecessarily apologetically by writing,

> A controlled clinical trial of psychogenic vomiters would be necessary to establish the efficacy of the overall therapeutic approach, as well as to clarify the important treatment components of such a multidimensional cognitive-behavioural intervention.

This loyalty to an inappropriate scientific model fails to do justice to the quality of the study and contains three conceptual errors.

The first is that he ignores the fact that the report as it stands provides an important piece of evidence for the effectiveness of the approach since it meets most of Kazdin's (1981) requirements. Secondly, even if the approach failed to be universally effective this would not invalidate the evidence from this particular case, since it is not merely evidence on the basis of induction but on the logic of the analysis performed at the assessment, the logical relation between the interventions chosen and the ongoing observation and analysis by the therapist of the interaction between the interventions and the client's thoughts, emotions and behaviours. Thirdly, the author assumes that the different treatment components could be identified and compared to each other in order to create a normative package. Yet the idiographic approach implicit in a behavioural functional analysis implies that treatments must be tailored to the complex of problems presented by particular clients, so that treatment interventions cannot be conceptualised as simply additive (Strupp, 1988: 80). This perspective is taken for granted in the best cognitive behavioural case law (e.g that for agoraphobia presented by Chambless, 1985, which is discussed in the next section. Biran (1988) demonstrates the danger of focusing exclusively on rigid treatment packages and limited behavioural goals in evaluating the outcome of therapy for agoraphobia).

Fox and Emery (1981) report an eight session therapy of a woman who had not had sex with her husband for seven years. This study, which includes extensive verbatim material, is essentially an extended case study since the full course of events over eight sessions is examined and a description given of the interaction between the content of the client's concerns, the therapeutic interventions and life events outside of the sessions. The way in which various developments in the relationship between the client and her husband contributed to the successful outcome is analysed within the general theoretical framework of cognitive and behavioural theory. It is also an illustrative-didactic examination of the application of existing case law. Finally it provides evidence for the value of specifically cognitive procedures by analysing the problems that arose in implementing a behavioural sex-therapy program, arguing for the appropriateness of cognitive interventions to solve these problems and demonstrating their successful implementation.

Edwards (in press, a), used two case examples to illustrate therapeutic work with early memories. He presented a detailed theoretical rationale in terms of cognitive developmental theory and linked the therapeutic technique employed to the logic of the psychological processes described in the theory. The main aim of the paper is to summarise and develop case law for this type of situation and the case material, in the form of two session transcripts, with commentary, serves as an effective focus for this, illustrating the phenomena being conceptualised and supporting the practical value of the theory presented.

An example of the development of extensive case-law: Agoraphobia

Extensive case-law has already been developed for a number of common clinical problems. It describes in detail the types of presenting symptoms and the manner in which these are related to habitual patterns of thinking and acting, and provides guidelines for assessment and the planning of therapeutic interventions. A particularly good example of this is the contemporary understanding of agoraphobia. Substantial research advances have led to an effective treatment approach through which up to 75% of agoraphobics may be substantially helped with fairly brief behavioural treatment, and, with up to 18 months of broader spectrum cognitive-behavioural psychotherapy, a larger number of complex cases can also receive relief (Chambless, 1985).

Three recent chapters encapsulate much of the accumulated wisdom concerning conceptualisation and treatment (Chambless, 1985; Coleman ,1981; Jasin, 1983). Each writer draws on personal clinical experience as well as the accumulated experience within the cognitive-behavioural approach. Agoraphobia is conceptualised within the broader context of the client's life problems, personality, and habitual patterns of thinking and acting and the principles upon which assessment and selection of treatment strategies are based are described and discussed. A common case-law underlying the presentations is evident. The content of this case law will not be summarised here; rather, some of the ways in which case study methodology is implicitly present will be discussed.

First of all, there is extensive use of "apt illustrations", especially by Chambless and Coleman. Here is an example from Coleman (1981: 104):

> Exploration of the spontaneous thoughts she had on entering these situations in the past revealed that mild anxiety symptoms led to the fear that she might faint. She was overwhelmingly afraid that this would lead to a trip to a hospital, a consequence she irrationally believed would lead to worse physical problems or to death.

This illustrates two well documented features of agoraphobics' cognitions: the tendency to misattribute the causes of anxiety symptoms and to fantasise catastrophic chains of events.

Secondly, extended case material is provided in the form of transcripts which illustrate whole frameworks of ideas within the established case law and provide evidence for its validity. For example Coleman (1981: 108-110) demonstrates a client's positive response to the therapist's use of standard cognitive and didactic arguments to persuade and encourage her to undertake a frightening homework assignment. Chambless (1985: 81-85) demonstrates how, in a complex case, various strategies were adopted at appropriate times to deal not only with the client's avoidance of entering public spaces, but also with social anxiety, lack of assertiveness, intolerance of being alone, issues about her religious affiliation and sexual values and finally feelings of abandonment that arose when her therapist accepted a job in another city. (For a fuller extended case-study that describes the practical problems of treating a complex agoraphobia see Sands, 1983).

From a formal point of view, much of the case study methodology is implicit. General principles or models are not documented point by point from case material: the chapters would be too long and cumbersome if they did this. This is an inevitable consequence of a research tradition where case descriptions do not form part of the corpus of research literature. However, if a greater wealth of published descriptive material existed it would form a valuable resource for supporting the validity of the many interlocking principles which these presentations offer. It would render explicit the basis of such case-law and would formally establish the place of chapters such as these as pieces of exceptionally good research writing.

Multiple case study for testing and developing theory: Obsessive-compulsive problems

Salkovskis' (1985) cognitive-behavioural analysis of obsessive-compulsive problems provides a fine example of the use of case-study method which is unusually systematic and thoroughly argued and documented. He developed a detailed model of the emotional, cognitive and behavioural processes that underlie the problematic behaviour based on a decade of literature which included three case studies of behavioural conceptualisation and treatment by Hodgson & Rachman (1976), theoretical analyses in behavioural and cognitive terms, and clinical trials of treatment procedures. He identifies key recurring features of obsessive-compulsive disorders. These, together with principles and hypotheses arising from the literature were incorporated in the model in a conceptually coherent manner and systematically re-examined against sixteen of his own cases.

The model describes ego-dystonic intrusive thoughts or images such as thoughts of contaminating others and spreading disease, or of making a mistake which results in later harm befalling others. These are followed by convincing and believable automatic thoughts such as "having these thoughts means I am evil", or "I'll be responsible for the suffering or death of others". The crucial thoughts imply responsibility for serious harm befalling others. Their negative emotional impact is enhanced by dysfunctional assumptions such as "having a thought about an action is the same as performing the action", or "not neutralising an intrusion is the same as wanting the imagined harmful event to occur".

The intrusions may be triggered by certain situations such as coming into contact with particular people or with supposed contaminants and may be countered by two major coping strategies: avoidance of the trigger situations (which gives rise to phobia-like behaviour) and neutralisation. Neutralisation can be cognitive (thinking a good thought after a bad thought, repeating magical formulas) or behavioural (checking, washing) and is reinforced because it reduces the anxiety associated with the intrusion and the automatic thoughts, and is perceived to have warded off an anticipated harmful event (children dying, house burning down) and the guilt which would have accompanied it. The completion of a neutralisation may itself trigger a new intrusion so that a new cycle of the process begins. The urge to neutralise will be accentuated by an existing depression or anxiety state since these render the automatic thoughts following the intrusion particularly intense and disturbing.

Salkovskis (1985: 580) provides an excellent example of the handling of a case which failed to fit the formulation: that of a girl who compulsively arranged objects in neat lines and found this a relaxing and pleasurable activity. Although she felt strong urges to perform this in times of stress it was not associated with thoughts of responsibility for harm. He extends his model to cover this by detailing a particular type of circumstance in which an initially neutralising act can become a self-maintained behaviour.

Salkovskis' model is not merely descriptive; it contains specific predictions that can be tested in new cases: 1) Intrusions that are not followed by automatic thoughts about responsibility for harm to others will not be associated with neutralising compulsions; 2) Intrusions are not followed by neutralising compulsions in depressed patients where the focus of depressive ideation is on helplessness and hopelessness, 3) Neutralisations are particularly intense in depressed patients whose negative ideation focusses on self-blame and guilt.. The impact of specific therapeutic strategies follows from the conceptualisation; for example, 1) cognitive intervention should focus on altering negative automatic thoughts and dysfunctional assumptions, not on eliminating intrusions, 2) patients must learn to stop avoiding the trigger situations and performing neutralisations, 3) depressed persons will not be successfully treated unless the automatic thoughts about being responsible for harm are thoroughly addressed. Given the clarity of the formulation and the extent to which it has already been tested against case material, the model provides an excellent basis for further detailed case research. Salkovskis and his students are already undertaking this (p. 582), but any therapist treating a case of obsessive-compulsive disorder is in a position to contribute in this area. If the case does not provide material which goes beyond the model this will be an exercise in applied science: gathering information about, investigating, and solving the problems posed by a particular case using an existing scientifically derived theory. The success of such an enterprise will further demonstrate the practical value of the theory and add to its external validity. If the case does not exactly fit, Salkovskis' model is sufficiently precise for it to be expanded or modified on the basis of careful descriptive work. This is not likely to challenge its overall validity so much as to extend its range of convenience.

Conclusions and recommendations

A number of recommendations follow from the material reviewed in this paper. The first is that presenters of courses and textbooks on research methods for general or clinically oriented psychology should either themselves have a clinical background or should have extensive dialogue with clinicians. Mann's (1984) otherwise well-written chapter on psychological research in Tyson's South African first year textbook entirely ignores the real foundations of clinical research method. It is perhaps not surprising since neither the author, Mann, nor the editor, Tyson, are themselves clinicians. Instead of dictating on ideological grounds what research is, writers on research should examine how knowledge is actually advanced and courses should give the case study method the substantial treatment it deserves.

Second, editors of journals should increase the practical relevance of the material they accept for publication and not routinely accept material just because it has a correct quantitative design (see Standing & McKelvie, 1986, for fuller recommendations in this regard). One way of redressing the balance would be to encourage researchers by having special sections or editions devoted to case studies, and, to invite well written case-study papers. Such a stand would facilitate implementation of the third recommendation which is that academics and post-graduate research students should produce considerably more case study research. Having good resources for publication would of course reinforce this process.

Fourthly, clinicians who are often too busy in practice to formally research their cases, should be encouraged to collect objective data (for example by tape-recording sessions) and to collaborate with full-time researchers. A post-graduate research student with access to therapy transcripts and regular case discussions with the therapist (and perhaps also the client - see Kruger, 1986) would be in an excellent position to produce carefully documented case study research. Such co-operation should be encouraged by research supervisors, and should be built into the structured activities of clinics which have research staff or are attached to universities. Perhaps the parameters of professional clinical psychology should more actively encourage clinicians to be genuine practitioners-as-scientists. They could be required to actively research their own cases or to collaborate with researchers before obtaining full professional registration or in order to obtain upgraded registration.

That some of these recommendations would be quite demanding both professionally and academically points to the fact that some balances still need to be redressed if the rift between scientist and practitioner is to be repaired. An ideological commitment and honest recognition of realities is required that is a major challenge for our discipline.

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370

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23

The phenomenological reduction and its relevance to human science and ideology critique

Fidéla Fouché

In this paper I shall discuss two related issues: (i) the Husserlian distinction between the phenomenological reduction's application by philosophers and by human scientists and (ii) two possibly conflicting tendencies in Husserl's account of the phenomenological reduction and, correspondingly, two conflicting interpretations of the reduction, namely those of Alfred Schutz and of Merleau-Ponty. I shall consider the first in relation to psychological practice and the second with reference to ideology critique.

The procedure of the phenomenological reduction

To begin with, however, it seems necessary to give at least a sketchy outline of the "reductions" or procedural steps or phases of the method which Husserl calls the phenomenological reduction.

The first step is the epoché or bracketing of the investigator's presuppositions both theoretical and commonsensical - presuppositions that is, both of the kind "the earth revolves round the sun" and "there is a world out there". The bracketing, setting aside or suspending of the theoretical beliefs one already holds about one's subject of investigation is, I think, generally recognised as necessary if the paralysis of dogmatism is to be avoided; it is a matter of being "unbiased" or "objective". Less familiar and rather more problematic is the notion of neutralizing one's commonsense assumptions about the world - or, in Husserlian terminology, suspending the natural attitude or standpoint, which is our naive faith in the objective existence of the world and of other people. When the natural attitude is bracketed the inquirer accords only phenomenal status to physical things, simply perceiving them and making no assertions about their existence. The purpose of the existential epoché is to liberate us

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from the naive epistemological belief that there *is* a world in-itself to which we can have access in such a way as to grasp it *exactly as it really is*. According to Husserl both common sense and natural science are victims of the natural attitude, the assumption on the part of both being that at least in principle scientific progress can ultimately give us a picture of the world as it really is. Husserl's implicit criticism of the philosophy of natural science is, admittedly, now somewhat outmoded - it preceded the work of Kuhn, Feyerabend, Popper, Lakatos - but it still applies in general to positivist conceptions of science.

The Husserlian reduction is *phenomenological* by virtue of the first step, the epoché, in that it leads us back to the phenomena, to "pure" experience. It is *transcendental* in that in another procedural step or phase it leads us to a residue which cannot be bracketed. For Husserl this transcendental residue is consciousness and it is the ultimate datum, given with apodictic self-evidence. The reduction is also *eidetic* in that, in a further step, it can lead investigators to the eidos or essence or meaning of their object.

The various aspects or phases of the reduction are mutually implicatory: the epoché is, for example, as essential for the reduction in its eidetic aspect as in its transcendental.

1. The application of the Husserlian reduction to human science

What has been said so far has relevance strictly to the phenomenological philosopher. According to Husserl only the phenomenological philosopher should bracket the natural attitude and suspend existential belief: the phenomenological human scientist should not bracket the natural attitude but only the presuppositions likely to hinder progress in his field. The human scientist is concerned with gaining understanding in his own field whereas the philosopher's concerns are more encompassingly epistemological and ontological. For the human scientist the transcendental reduction does not apply for the same reason that the bracketing of the natural attitude does not apply, namely that the search for a philosophical a priori is of interest to philosophers and not to human scientists qua human scientists.

Husserl pays far more attention to the difference between phenomenological philosophy and psychology than to the difference between phenomenological philosophy and human sciences other than psychology. The reason is that phenomenological philosophy and non-naturalistic psychology have in common a central theme, namely subjectivity. There is for Husserl, however, a methodological distinction between the two: phenomenological philosophy brackets the natural attitude both on the common sense and on the theoretical levels, phenomenological psychology does not bracket nature as immediately given by common sense but it does exclude all theoretical concerns with the physical from its theme, which is of course the psychical. In *Phenomenological psychology* Husserl says that the pure psychology he envisages, which is closely related to phenomenological philosophy, has not hitherto existed. What has gone by the name of psychology has been an exploration of the psyche on the model of natural science: "the psyche as spatialized and objectively temporalized in physical nature as a causal annex of the physical body" (1977: 166). Husserl believes that the projected psychology he allies with phenomenological philosophy should be the fundamental socio-cultural science. Where natural science excludes the "merely subjective" and is directed only to extended reality and to establishing objective, universal truths about it, socio-cultural science is directed solely toward the personal. Spatial things are included in the socio-cultural only as belonging to the personal surrounding world. Husserl notes that the term "surrounding world" has a personal meaning, while "nature" is a word which has excluded all personal meaning. Psychology as socio-cultural would not posit nature at all, and instead be "thematically orientated toward *human persons* in their personal nexus or toward their personal surrounding world" (1977: 169). Inseparable from persons is their surrounding world "even if it may contain specters, fetishes etc" (1977: 174)

In *Crisis in European thought and transcendental phenomenology* Husserl repeats that the phenomenological psychologist should remain in the natural attitude as far as his assumptions about the objective existence of his subject are concerned. While the psychologist should see his subject and the world as transcendent i.e. as actually existing, he must "put out of effect the totality of his participation in the validities explicitly effected by the persons who are his subjects" (1970: 239) i.e. make no evaluative judgements about his subjects' account of their experiences. For his role is that of "the absolutely disinterested psychological observer" (1970: 240).

Critical comments

The phenomenological psychologist will take it for granted that his subject is real but he will refrain from judging whether her experiences have a reference to reality. While Husserl is concerned primarily with a psychology which is to be the foundational science for all of the socio-cultural sciences it does not seem unfair to put his assertions about the reduction to the test by referring them to a clinical psychological situation. Husserl's intention is obviously to emphasise that the psychologist's relationship to his subjects should be one of empathy and not of manipulativeness but his psychological epoché which brackets the psychologist's forming judgements about the validity of his subject's experiences can possibly give rise to absurdities. Indeed it is difficult to see what good purpose can be served by the psychologist's taking for granted the physical presence of his patient while at the same time maintaining an open mind as to whether she is truly persecuted or paranoid - and this *in spite of positive evidence* which under normal circumstances would settle the matter. Suppose a subject claims that she is constantly pursued and attacked by birds. The psychologist sends his agitated patient into the garden to calm herself and he observes a swooping and sqawking of birds. If he were a consistent Husserlian he could not allow his perception of the real birds to influence his understanding of his patient's state of mind; he could not question whether her surrounding world was populated by spectres and fetishes or by real, physical beings. Such simulated ignorance would surely be not only absurd but also counterproductive.

Merleau-Ponty's version of the reduction

Merleau-Ponty rejects the Husserlian suspension of the natural attitude and he does not prescribe a modified version of the reduction for human scientists as opposed to philosophers. In "The Philosopher and his shadow" (in *Signs* 1963) Merleau-Ponty distinguishes between the *natural* attitude and the *naturalistic* attitude. The natural attitude is natural to us in that we spontaneously believe in the existence of the physical world and of other people. The naturalistic attitude, on the other hand, arises from a theoretical adherence to materialism (1964: 163). Since the term "attitude" strictly implies a position deliberately adopted, our spontaneous existential belief does not arise from an attitude. Merleau-Ponty therefore substitutes for "natural attitude" the phrase "perceptual faith". Perceptual faith does not arise from the positing of a thesis but rather forms the implicit presupposition for all theses. The Merleau-Pontyean epoché does not, therefore, attempt a bracketing of the perceptual or pre-theoretical: it *does* bracket the naturalistic thesis that the world *is* quite univocally as it presents itself to an impartial viewer. According to him the human scientist, like the phenomenological philosopher, should, thus, apply the epoché only to cognitively held or theoretical views and not to existential belief.

This modification of the Husserlian epoché does away with possible absurdities of the kind I mentioned above. In the Merleau-Pontyean version, psychologists, for example, will apply the epoché equally to their subjects and to their subjects's experiences; they will not doubt that their subjects physically exist but that they *are,* in any respect, exactly as they have been assumed to be. This version involves a more open, tentative attitude on the part of psychologists both to their subjects (e.g. as to whether they are lying or telling the truth) and to their subjects' experiences (e.g. whether these experiences are perceptual or hallucinatory). Where the Husserlian epoché would prescribe a noncommittal attitude on the part of the psychologist about his subject's *experience* of vengeful birds while he would not need to be sceptical about his own visual evidence of their ontological status, a Merleau-Pontyean psychologist would not be faced with the curious distinction between attitudes he should adopt towards what he sees and what his subject believes she sees. Merleau-Ponty decisively does away with any dichotomous separation of subjects from the real, intersubjective world.

2. The contrary tendencies in the Husserlian reduction

Conflict may arise from unequal emphases given to two of the phases of the reduction: the negative step of the epoché which is one of excluding presuppositions, and the positive step which follows, of observing and describing one's object in an unprejudiced way. The epoché, designed to make possible a new beginning which is uncontaminated by unexamined presuppositions, can be seen, as Husserl and others often do, as aimed at achieving indubitability - whether "adequate" in the sense of empirically grounded or of "apodictic", logically self-evident. Indubitability can be seenas primarily a natural scientific and Cartesian ideal and as incompatible with the rich and complex evidence yielded by phenomenological description and which we associate with the arts rather than with natural science. When Husserl speaks of the role of the philosopher or human scientist as being that of the "disinterested spectator" and of phenomenology as a rigorous science, a proto-science, the science of sciences (Husserl, 1970) he is clearly following the ideal of indubitability and emphasising the negative aspect of the reduction; the philosopher abstains from bias, philosophy admits nothing unexamined. Philosophy is the proto-science because it alone operates on the basis of certain evidence; by virtue of the epoché de facto sciences have become merely alleged sciences (1973: 8). But when Husserl says of the epoché that "it is through this abstention that the gaze of the philosopher first becomes fully free" (1970: 151) he would seem to be describing the positive aspect of the reduction - the reduction, that is, as opening the way to a complex, many-dimensioned apprehension of the world, rather than as a way of establishing indubitability. It is true that the goals of certainty and a richly nuanced apprehension of the world are incompatible only if we believe the certainties attained by human beings as final. If, however, we hold (adequate) certainty as only provisional as indeed Husserl does e.g. when he says that what is evident can subsequently become doubtful and that being can prove to be illusion (1973: 15) - there is strictly no incompatibility between the two goals. For the discarding of presupposition points towards the making of a new beginning in which one makes no use of previously held knowledge and making a radically new beginning means a prior discarding of past acquisitions. Seen in this way the reduction suggests a progressive illumination rather than a resting upon indubitable truths. In his late work, moreover, Husserl is highly critical of what he calls "the positivistic reduction of the idea of science to mere factual science" (1970: 5) which excludes questions Husserl finds "the most burning", namely "questions of the meaning or meaninglessness of the whole of this human existence". Science (as natural science), Husserl observes, has nothing to say about reason, unreason and freedom (1970: 6).

Yet there is a tension between different tendencies in Husserl's account of the reduction. It remains more or less hidden in his work and becomes explicit in the different interpretations given to the reduction by his successors. Schutz for whom the phenomenological investigator is above all the "disinterested observer" represents those who give a negative meaning to the reduction. Merleau-Ponty most strongly of those successors of Husserl who did not reject the reduction takes the opposing non-naturalist line.

Schutz's understanding of the reduction

Richard Bernstein in *The restructuring of social and political theory* focuses on Alfred Schutz as Husserl's most representative follower in the field of phenomenological sociology and he makes criticisms of Schutz which he generalizes to a critique of phenomenology in general. One of his criticisms is that phenomenology because of its insistence on a disinterested stance cannot assess any one historical form as dehumanizing, alienating or repressive and that therefore it is incapable of providing a critique of ideology (1976: 168). I hope to show that Bernstein's criticism is applicable to Schutz only and not, for example, to Merleau-Ponty.

Schutz does not agree with Husserl that the human sciences are *toto coelo* different from the natural sciences and he aligns himself rather with mainstream social science in that he - like Nagel and Hempel - sees many correspondences between the natural and human sciences on points of procedure. He does agree with Husserl that there is a fundamental difference between the subject matter of the natural and human sciences, which is relevant to the way we approach them. The natural scientist deals with facts, data and events relating to molecules, atoms and electrons; the facts, data and events "mean" nothing to the atomic and subatomic particles. The human scientist deals with human beings and with a social world which they *interpret* by way of common sense constructs. The constructs used by human scientists are "constructs of the second degree", namely constructs of the constructs made by the actors on the social scene (1962: 6). Phenomenologists cannot but agree with Schutz that there is a relevant difference between the subject matter of natural science and that of human science, but there is nothing distinctively phenomenological about this view. Indeed Schutz acknowledges his debt to Weber's concept of *Verstehen* which has also influenced philosophers of social science belonging to the Anglo-Saxon analytic tradition. Peter Winch, for example, following Wittgenstein, strongly argues that the social scientist's concern should be conceptual analysis and not causal explanation.

Schutz's insistence that the human scientist *like the natural scientist should* adopt the neutral position of the disinterested observer is likewise more Weberian than phenomenological. It one-sidedly emphasises the negative aspect of the reduction: "All science presumes a special attitude of the person carrying on science; it is the attitude of the disinterested observer" (1962: 249). The scientist as a disinterested observer takes no practical interest in the world, does not participate in it, brackets his values. He does not bracket the natural attitude. The neutrality Schutz demands from human scientists is, by his own admission, no different from that demanded of all scientists. In other words he follows Weber's precept that human science, like natural science, should be Wertfrei.

Schutz in various contexts insists on the ultimate authoritativeness on the matter of procedure of the science the investigator practises. He says, for example, that the social scientist observes the behaviour of the actors on the social scene and "tries to explain it in accordance with the procedural rules of his science" (1962: 6); he equates "scientific" with "objective" and "objective" with "conforming to the canons of verifiability and testability by the scientific community" and he emphasises that the latitude for the scientist's discretion is very small. And he says further: "The stock of knowledge at hand is the corpus of his science and he has to *take it for granted* - unless he makes explicit why he cannot do so" (1966: 39). Schutz has obviously embraced a conservative natural scientific ideology and here he seems to depart radically from a salient feature of Husserlian procedure. For the phenomenological reduction is a device, precisely, to ensure that *nothing* be taken for granted, that everything be critically examined including the foundations of one's own science. Indeed Husserl says *à propos* of the reduction: "And so we make a new beginning, each for himself, with the decision of philosophers who begin radically; that at first we shall put out of action all the convictions we have been accepting up to now, *including all our sciences*" (1973: 7) (my italics).

Can phenomenology provide ideology critique?

I wish now to consider Bernstein's view that phenomenology, because of its insistence on the neutrality of the scientific investigator, cannot provide an ideology critique. Ideology critique would indeed seem to fall outside of the scope of Schutz's impartial elucidation of social structures. The "disinterested observer" in placing him or herself "outside" of ideology is an unwitting and therefore more seriously deluded adherent of a positivist type of ideology.

Merleau-Ponty is highly critical of Husserl's frequent emphasis, in his earlier work, on the epoché as producing a purely theoretical attitude on the part of the subject, and, correlatively, leading to an ontology of "blosse Sachen", things simply as things, "stripped of every action-predicate and every value-predicate" (1964: 163). For Merleau-Ponty - as I have already said - the reduction takes us to the perceptual world as the transcendental residue that remains outside of the brackets and not to a pure subjectivity. Clearly the world we perceive is not a neutral fact-world which acquires values only when we make value judgements at the level of cognition. Rather, since the perceptual world is also, for human beings, a cultural world, it is "given" to us already endowed with value, and our basic evaluative attitudes are formed at the same level as that on which we "synthesise" the world. Or, expressed yet differently, (as Ricoeur, following Geertz puts it) the primitive structuring which articulates our experience is by no means ideology-free (1984: 11/12). Merleau-Ponty's theory of a precognitive or antepredicative genesis of meaning (which develops rather than conflicts with Husserl) implies a recognition of the deeply ingrained nature of our fundamental values without, however, suggesting that there can be no remedy for this uncritical harbouring of values. In considering the reduction's effect on the philosopher's (and by implication the human scientist's) procedure he speaks, following Husserl, of the philosopher as a perpetual beginner, taking for granted nothing that others believe that they know. Here he has in mind a bracketing of theory. But he goes beyond advocating a suspension of theory, of explicitly held judgements, when he speaks of the reduction as *radical reflection*, reflection on the unreflected, which leads to "a consciousness of its own dependence on an unreflective life" (1962: 56 - 63). Merleau-Ponty has in mind the kind of reflection which seeks the genesis of our taken- for- granted perceptual world but which equally has relevance to the origins of our values. Reflection on the unreflected means a making explicit of the implicit. The explication of the implicit is critical activity, what Husserl calls *Wesenschau* or eidetic reduction. It is also a hermeneutic process. A methodically applied epoché of what we thought we knew gives rise to a sense of puzzlement, of no longer seeing our knowledge as translucently self-evident. And this in turn brings a need for clarification, or in Husserlian terms, for eidetic insight. With the eidetic reduction it is, Merleau-Ponty says: "a question of replacing habitual concepts to which we pay no careful attention, by concepts which are consciously clarified" (1970: 61). And further: "The insight into essences rests simply on the fact that in our experience we can distinguish *the fact that* we are living through something from *what it is* we are living through in this fact" (1970: 49). The transition from merely living through an experience and questioning the nature and significance of that experience is a transition from the unreflective to the reflective.

I shall not consider issues like Mannheim's paradox (1986: 8, 9 etc.): how we can have a critique of ideology which is not itself ideological. I shall assume - provisionally of course - that our only weapon against ideology is a perpetual questioning of the meaning of social phenomena. If the epoché is taken not as a means of insulating the investigator against interest but rather as an abstention from present bias *in order to* open up other perspectives, other possibilities, he or she will not be a Schutzean "disinterested observer" by definition incapable of evaluation. Moreover the various steps of the reduction will be seen as implying the others. When the epoché - and thus the purely theoretical attitude - are absolutized the phenomenological method is fragmented and the dialectical relationship between the phases obscured. This means that there is an arrest atthe phase of epoché and "objectivity" becomes an end in itself instead of a means.

Not only is the relationship between the steps of the reduction itself dialectical but there are also, it seems to me, important parallels between the phenomenological and the dialectical methods: both methods encourage a sensitivity to ambiguities and tensions, both take account of historical relativity, both challenge what is passively taken for granted, and both can accommodate apparently conflicting ideas. To illustrate, let us take the belief that the Freedom Charter is vague, Utopian (in the bad sense) and that it inhibits the working class movement. An investigator who follows the dialectical method may confront this thesis with a challenge to the effect that the popularist movement of which the Charter is an expression is essential for rallying the wide support necessary if apartheid is to be destroyed and that this is a precondition for the transformation of the social relations of production. A phenomenologist who brackets a similar preconception equally admits an alternative view. The point is that both dialectical and phenomenological methods aim at bringing about an understanding of situations in a way which accommodates and integrates their opposing aspects.

It might justifiably be claimed that it hardly takes an allegiance to the phenomenological reduction or the dialectical method to describe situations in their complexity: Duncan Innes achieved precisely this in his article on the Freedom Charter without obviously subscribing to either method (1986: 41). Phenomenologists and dialecticians obviously do not have a monopoly of the methods they propound. The virtue of the phenomenological and dialectical methods is that they prescribe procedures designed precisely for grasping the complex and dynamic as such, as opposed to understanding by way of the analytic method which elucidates by isolating discrete elements. Self-consciously to adopt any of these methods is to announce one's approach to reality.

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24

Awareness of the self in relation to others: A discussion of the method and findings of an empirical phenomenological study

Trevor M. Hoek

Insight, in traditional psychoanalytic therapy, could be defined as "the total understanding of the unconscious determinants of one's ... feelings, thoughts, or behaviours ..." (Phares, 1984: 394). The emergence of such an understanding for a person is explored in this paper as an interpersonal event, in a similar way to Kruger's (1988) understanding of interpersonal insight, in the case study of "Sara", where he writes that:

> ... in a sense, we can say that Sara was conscious of her actions but... she did not see what the significance of those actions were ... [sic] By achieving ... insight she was sharing a level of consciousness that was already present in some people around her, notably her therapist. What was "unconscious" in the life of Sara was conscious in the life of people with whom she was interacting (Kruger, 1988: 131).

Following Kruger, then, that which is unconscious and then emerges into consciousness as "insight" or "insightful understanding" is contained in the relationship between the person and others. Such "insight" is perhaps similar to that phenomenon which Buber (1947) refers to as "becoming aware", an event which he also writes of in interpersonal terms. Buber notes that becoming aware is a different kind of perception to that involved in "observing" or "looking on" it is a different matter to such an aloof perspective when, in a "receptive hour" of my personal life someone "says something" to me, and addresses something that enters my own life, even though that person has indeed not noticed me at all (1947: 9). This can perhaps be seen as "being addressed by *oneself*" while involved with other people. Parker (1985) refers, in these terms, to the psychological notion, derived from Heidegger's work, of the *call to*

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oneself from oneself, or "conscience", and it is the basis of coming to an authentic self-understanding.

Insight, as a "becoming aware", authentically, of who one is, can also be understood in the light of Heidegger's account of the "clearing away of concealments and obscurities" (1962: 167) when a person takes up his or her Self in his or her very own way. For Heidegger this is an interpersonal issue, since for him, the reason for the need to take hold of oneself in one's very own way, is to be found in the fact that others initially effect a tyranny over one; dominating one's thoughts and ideas about oneself, and in a way making choices for one. In Heidegger's terms, any definition of the "individual" must include the clause that an individual is "he, or she, who is his or her own *path of choosing*" (1962: 33, 168). There is, though, another aspect of "the interpersonal", since, for Heidegger, that which is chosen by a person and then taken up in a unique way by the individual, is chosen from among *the possibilities manifested by other people* (ibid.). Every authentic person is a unique configuration of these general possibilities. Excessive "sameness" is for Heidegger the epitome of inauthenticity (1962: 163-164).

"Insight", in this paper, is explored as a becoming aware of who one "is" in a genuine way. It is also a special kind of perception of oneself that has explicit and implicit interpersonal dimensions that lead toward the "clearing away of concealments and obscurities" (Heidegger, 1962: 167) and the experience of genuine conflict through seeing important disjunctions, for example between "who one really is" versus who others think one is, or between who one is versus who one can be (Laing, 1971).

The research which this paper reports on was structured in terms of the guiding question: "what is the psychological structure of realizing something about oneself, through perceiving someone else?"

The methodology used in the project was the psychological-phenomenological method of textual analysis, carried out on interview data.

Pilot Study

The central interview question, i.e. "Could you describe as concretely and in as much detail as possible, an experience in which you had a leap of insight into your life-situation because of some way that you perceived someone else?" was long and complex. The question was thus first piloted. One person from a group of thirty first-year students contacted the researcher who then interviewed her. The interview was recorded on audio tape and transcribed. The interview was found to be an adequate description of an insightful experience, since the person had come to discover that she structured her life too rigidly around her work in order to meet the expectations of others. The question, although difficult, was thus able to elicit the intended type of responses.

Subjects

The criteria for selection of the subjects were that:

(a) the subjects could relate an experience such as the one outlined in the interview question.

(b) in order to ensure a baseline level of clarity, the insight gained should be experienced through perceiving one other person. Two of the six interviews conducted (excluding the pilot study) were rejected on the basis of this latter criterion. In the end, in addition to the data collected in the pilot study, four subjects' interviews were analysed.

Interviews

The researcher interviewed each subject separately. Each interview was recorded on audio tape and then transcribed. Aside from the initial question, which was the same as the piloted question, questions and comments were restricted to requests for clarification, consisting mainly of reflecting back to the person, what he or she had already said, or restating the essence of the initial question.

Data analysis

The interviews were analysed using the psychological-phenomenological methods developed by Giorgi(1975, 1985) and Wertz (1985).

Each complete description was read and re-read in order to understand the whole experience. The researcher then went on to break up the text into manageable units according to shifts in psychological meaning, e.g. a shift from a description of the general context to a specific reference, by the subject, to his or her own feelings. These units were then re-written and expressed in the third-person e.g. "S did such and such" instead of "I did such and such". The researcher then transformed each of these units with the intention of interpretably re-stating and amplifying the meaning which the subject intended. The interpreted segments which pertained to each other were then grouped together. All the units were also ordered to reflect the course of each subject's experience through time. The researcher then interpretably amplified the data from each subject: just as each unit had been amplified, the researcher now amplified the whole text on the strength of the prior unit analysis. This was written up as a case report which reflected each specific subject's insightful situation; the report, at this stage, still included much of what was incidental to the specific context. The researcher then read each of these "specific situated structures" in order to identify what seemed most pertinent to defining or explicating the *phenomenon* being researched, and to exclude that which was incidental to each subject's context; this analysis was then written up to reflect that which was typical of the phenomenon, in an explicatory[1] sense, for each case. The final general reports for each case were then all compared and contrasted to arrive at descriptions of the defining themes which constitute an understanding of the *meaning of the phenomenon* of insight, especially as manifested through perceiving another person.

Results

The results of the research are presented in two parts. Firstly, definitional descriptions of (a) "insight" per se and (b) insight gained by perceiving another person are presented. Secondly, the definitions are elaborated around specific interpersonal themes.

Part 1: General description of insight, especially insight gained by perceiving another person

(a) *Insight* occurs when a person is struck by a new way of perceiving an aspect of his or her life. This new way of perceiving such an aspect can include a feeling of guilt about how the person has been behaving, but specifically reveals a better, more real way of perceiving the issues in question. Insight involves not only a powerful perception of reality, or a being "struck" by reality, but also a clarity of perception. This *clarity* is so strong that the person not only sees *into* his or her situation, but also sees right through it, to beyond the immediate context. In these terms the person looks back into his or her past, as well as forward into the future. Thus, for insight to be truly "insight", it must mean that the person's outlook on the past and present changes, and that the person takes up new *projects* to make real the new, more authentic view he or she now has on his or her life.

Insight also involves one's bodily reality, to the extent that (i) during the experience of gaining insight the person feels a deepening of emotions, and (ii) the person becomes aware of new feelings towards the world and towards others.

(b) *The interpersonal situation* in which the person gains insight by perceiving someone else has its origins in the person's being intensely involved in his or her inauthentic life, and lost in the immediacy of his or her concerns about his or her projects and tasks. These projects and tasks have been moulded through a taking up of the abstracted values, demands and orientations of others, who perhaps have also been abstracted.

Then, however, the person encounters someone whose presence is so powerful, especially in terms of the subject's inauthentic concerns, tasks or projects, that it calls the subject away from the immediacy of his or her being concerned, towards being intensely and reflectively concerned with that other person, or towards an "other" new way of being. The other person thus draws the person out of a rut as it were, so that the person perceives differently, e.g. discovering that he or she is already behaving in an *authentic* way which is different to that which he or she believed possible; or conversely he or she guiltily discovers him or herself to be behaving inauthentically, in ways *contrary* to his or her genuine interests. This encounter thus invokes conflicts and polarities.

The person, having then seen or "experienced *for himself or herself* another, conflicting, way of perceiving or experiencing, feels compelled to bring his or her own beliefs or actions into question in the light of the reality that is discovered.

The other person who had so moved the subject also recedes into the background and becomes a part of a newly opened *world* in which people appear differently, i.e. they are perceived as more important and more real. An atmosphere of clarity, reality and openness prevails. The person in his or her guilt, experiences a sense of having discovered truth, and a sense of having learned something.

Part 2: Extended description involving an elaboration on important interpersonal themes.

We now move on to an elaboration on important interpersonal themes which were found.

A: Before the encounter there is inauthentic concern about how one stands in relation to others

Although the danger of being "inauthentic" lies in the concern about how one stands with respect to others, it is in this "concerned reflection" that the saving power lies, since it is this very being-reflective in the face of others and oneself that allows the person to take up a different perspective.

B: The encounter with another distracts the person from the immediacy of his/her uncritical self-concern

The other person becomes so intensely present to the subject, in order to call the subject to beyond his or her immediate, intense, living *in* his or her projects, fears or concerns. The person is "called away" to reflect on the other person, or to reflect on another way of being revealed to him or her by the encounter. Thus, we can understand the experience of Subject 2 whose encounter forced her to re-think her standpoint; she notes:

> And in that moment it occurred to me that maybe I have [sic] been wrong all the time, and that maybe he was in fact very unhappy about something. And all the time I had assumed it was directed at me - so it made me think ... look through all my other reactions to people as well.

C: The Seeing of the contrast between oneself and the other, and/or between one's own uncritical assumptions and one's own new behaviour

The person finds himself or herself to be standing in two places at the same time. Having been transported from the immediacy of his or her everyday inauthenticity, he or she discovers him or herself to be living a contrasting understanding. The person authentically discovers the very extent to which he or she had been, or is, living inauthentically. This understanding emerges as a contrast between how the person is living and was living. The person also discovers that his or her new way of living contrasts with what he or she had previously believed or thought.

An example of this embodiment of an *other* way of being that is in contrast to the subject's own prior way of living, is Subject 3's experience in which he discovered that he was no longer in a panic:

> I was really worried ... And at that moment R... came up to me and put his arm on my shoulder and said ... "don't worry ..." and ... we both laughed ... And when I stopped laughing ... it struck me.

Subject 3 notes, then, that after his encounter "instead of just looking at these people ... in terms of fright ... I experienced a sense of calmness".

The realization of how one is, or *can* be, is lived in these terms as a sense of *discovery.*

D: Seeing differently, especially seeing others differently

The other person who had moved the subject towards a new understanding becomes a part of a newly opened *world*. That "other" whom the person encountered, together with other people in general, are perceived as more important or more real. For example, Subject 1 notes: "I saw B... differently. B... added a depth to himself... like he moved out of a two dimensional mould".

Discussion

Rycroft (1972) notes that "insight", in psychoanalysis, refers "in the first instance to *self awareness* and self-knowledge, (but) it is also used to refer to the capacity to understand others" (Rycroft, 1972: 73). For example, Subject 2 noted: "And that incident made me realize that I have been wrong; that you *shouldn't take things so seriously you know. That I should be more accepting* of people".

This quote also reflects Van den Berg's point that the unconscious of the person may be said to be located "not inside the client, but in the world... characterized by the relationship between the (person) and ... others" (Van den Berg, cited by Kruger, 1988: 132). Furthermore, the quote from Subject 2's description also suggests to us, as does Parker (1985), that in similar terms to the phenomenon of conscience, which he defines as a call to oneself from oneself, "insight", is a calling of the person away from *being guilty*, so that things can be seen as they really are. There is thus a strong empirically derived link between insight and conscience, i.e. between the *call* to see reality and the subsequent "seeing" of things as they really are.

Parker (1985) also notes that "it is difficult to conceive of an experience of conscience, as a call to oneself from oneself, which does not involve others" (Parker, 1985: 182). In these terms we will go on to discuss insight, which following Norman (1978), we also define as "seeing familiar things, especially those in our own personal world, "in a new light, as though for the first time"" (1978: 327). We will do this in terms of the interpersonal issues discovered in our study. This discussion of insight will be presented in terms of two themes: (a) awareness of the self, and (b) awareness of others.

(a) Awareness of the Self

The structure of the emergence of "self-awareness" in terms of perceiving another person, is a clear example of the essentially intersubjective nature of the self as it lives in everyday situations. Heidegger's (1962) view is that the human being is, itself, an "other", or "one among others" (Theunissen, 1984: 184), so that becoming an *individual* among others is the result of choices from, and modifications of, possibilities revealed by others and a movement away from standing "in subjection to others" (Heidegger, 1962: 164). For example, we note that Subject 2 was able to move away from such subjection when she states:

> And in that moment it occurred to me that maybe *I have been wrong* all the time, and that maybe he was in fact very unhappy about something. And all the time I had assumed it was directed at me - so it *made me think* ... look through all my other reactions to people as well.

Clearly the experience also constituted what Buber (1947) termed a "meeting" or encounter: and in this type of perception, the other is certainly not an object. Subject 2's experience then mirrors Buber's account of when, "in a receptive hour... a man meets me about whom there is something ... (which) says something to me, addresses something that enters my life..." (1947: 9). Szczepanski (1979) notes also that the self, at such times, is able to encounter the other in the world of relations between people "in themselves" (1979: 123). Subject 2 had encountered the person she spoke of *in his own right*! Our study also found that it is the person's very concern for the appearance of others in his or her life which allows the other to become powerfully present to him or her in a moment of conflict, contrast and polarity. This "otherness" that addresses itself to him or her calls him or her back to him or herself: just as Cooper (1983), citing Merleau-Ponty, notes: "The body knows itself only through taking the position of another through which *it comes back to itself*" (1983: 203, my italics).

An example of the embodiment of another way of being that is taken up in contrast to the subject's own prior way of living, was seen in Subject 3's experience, when we quoted from his description of his discovering how calm he could be, and indeed was; he noted:

> I was really worried ... And at that moment R ... came up to me and put his arm on my shoulder and said ... "don't worry ..." and ... we both laughed ... And when I stopped laughing... it struck me.

"Otherness", thus, can paradoxically emerge as an already owned, *but contrasting* "mineness".

Laing (1971) writes that for any encounter to be therapeutic, or allow the person to become aware insightfully, it is necessary that such disjunctions be seen (1971: 143). The disjunctions between ways of living, which become obvious, serve to clear up falsehood and confusion: "... when the issue is false and confused ... "true" choices are not available" (ibid.). Laing (1971) also writes that "being-real" or realizing oneself is a losing of oneself from where one was, and thus is a making "patent of the latent self of the doer" (1971: 126).

It is also clear, from our findings, that the role of what Leahy and Shirk (1985) term the "social-self concept" in the development of authentic selfhood, is, in part, a negative one. Conformity, imitating others, and being overly concerned with how others relate to one, all restrict the person, until he or she is thoughtful enough, or perhaps has suffered sufficiently. Then the person can come to see his or her position as an individual clearly. This process represents an increase in integrity which also results in the person being increasingly open and "variable across situations in time" (Leahy and Shirk, 1985: 147). The person experiences an increasing level of integrity when there is a "separation of self from its social sources" (1985: 131).

b) Awareness of the Other

The person who experiences a becoming aware of who he or she is in him or herself also experiences the other person, including the person whom he or she encountered, in a new and different way. Laing (1971) wrote: "It is impossible for me to maintain a false picture of myself unless I falsify your picture ..." (1971: 143), and Szczepanski (1979) wrote conversely: "If I am aware of my essence as my individuality, then I am aware ... that there must exist ... relations between people ... 'in themselves'" (1979: 123).

The person thus gains the freedom to experience others in a different way. For example, Subject 2 in our research noted: "It made me realize ... you should be more accepting of people ... (and) I felt I could be more open ... within my own little group." Halling (1983) also notes this sense of openness which he found when his subjects described an experience in which they saw another person as if for the first time:

> ... in responding to the new in a situation, one becomes a free person. In fact, the situation in which one sees the other "as if for the first time" is one that in its perceived newness renders irrelevant, inaccessible or inappropriate previously taken for granted ... ways of interacting with or understanding the other person (1983: 126).

Conclusion

In conclusion we note that this paper has outlined the link between conscience, as a psychological concept defined as "the call to oneself from oneself" and insight. The paper also explored the interpersonal issues relevant to these two concepts, indicating the strong possibility that the capacity for insight depends on the capacity to entertain points of view other than one's own. This latter capacity, in turn, also depends upon the capacity for concernful reflection on one's own personal reality, which is, first and foremost, a shared reality.

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Note

A.R. Lacey notes in *A dictionary of philosophy,* that: "Explication, when not simply a synonym for "explanation", is the process whereby a hitherto imprecise notion is given a formal definition, and so made more suitable for use in formal work" (1976: 66). The aim of much work in phenomenological-psychology is to do this empirically.

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25

The question of knowing and truth in psychology and psychotherapy

Dreyer Kruger

Psychology in defining itself as a science, thereby implicitly and explicitly defined itself as an enterprise that seeks to arrive at generalizations, comprising statements which would have validity for a class of events. However, whilst introductory psychology textbooks very often thematize the idea that psychology is a science, the question of how truth is to be attained, is not thematized. Thus psychologists and psychotherapists do not systematically address the question of how true statements in psychology are constituted and whether current conceptions of truth require modification in a systematic psychology which is truly concerned with its subject matter. This state of affairs is, generally speaking, symptomatic of contemporary psychology: in its unrelenting effort to be a natural science, it is so obsessed with scientific respectability that it leaves itself no room to question either the metaphysical conceptions of the science itself nor the Cartesian rationalist conception of truth to which the natural sciences are oriented, i.e. the so called correspondence theory formulated as *adaequatio intellectus ad rem.*

Partially as a result of this lack of questioning psychologists have to restrict themselves to various measures of validity which have not been adequately thought through. They have tried to get an objective way of validating quite subjective ways of measuring, e.g. personality questionnaires giving an indication of some intrapsychic state e.g. accident proneness, the results of which are then correlated with actual accident statistics or measures of the severity of accidents. The ideal is to find "hard data" to validate intelligence and personality and various other measurable constructs and also to root psychology in laboratory work. Clearly these strategies have not been successful. Psychologists have failed to question the conception of truth itself and to ask themselves whether their validation efforts are not simply ways and means of trying to find an Archimedean point outside the universe of human scien-

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tific discourse. In the insistence of psychologists on these quantitative approaches that seek for outside criteria of validity we see the influence of the philosophy of positivism which has dominated psychology for most of the century and continues to do so.

The aim of this paper is to defend three propositions:

(a) The rationalist-scientistic conception of knowledge is inadequate as a basis for accounting for the nature of disclosure in psychotherapy and, by implication, in psychology as a human science.

(b) Coming to know in psychotherapy is not exclusively a cognitive process and more specifically it is not a *purely* cognitive process even when it is cognitive.

(c) The Cartesian conception of truth is unsuited to describing the reality of the inter-human encounter typified by dialogical psychotherapy and should be discarded in favour of a theory of truth as disclosure.

As regards the above, I may comment as follows:

(a) Modern scientific practice in psychology and psychotherapy is dominated by the idea of the objective. This means that pure knowledge is supposed to be divested of all elements except those which characterise the object itself. However, in Cartesian philosophy as well as in contemporary scientific practice, the basis of certainty is centred in the subject who dictates to the object that it shall be known only in terms of extension. This means that we basically have the almighty subject prescribing reality to the world. This is a thinking which is basically oriented to mastery of the world (*Herrschaftsdenken)* and provides the basis of contemporary technology and technocracy. In psychology, this basic technological attitude is expressed in behaviourism, cybernetics and systems theory and, in psychotherapy, in behaviour modification and rational emotive therapy to mention just two examples. However, this attitude is disputed at a basic level by the reality of the very structure of most dialogical (e.g. psychoanalytic, Jungian and Daseinsanalytic) therapies. These latter approaches cannot function if it is insisted that statements are to be made concerning clearly definable and measurable objects (i.e. objectified subjectivities) (Kruger, 1983). On the contrary, these approaches are hermeneutic (Kruger, 1988a), i.e. they are oriented towards meaning and not towards objects in the Cartesian sense. Meanings tend to be multiplex or multivocal rather than univocal and at the same time reflect the ambiguity of life. There is no way in which meanings can be operationalised into simplistic statements and it is quite clear therefore that the hermeneutic approach cannot, as far as its epistemological conceptions are concerned, fit into the Cartesian rationalist subject-object scheme. The very practice of psychotherapy, to my mind, brings this out again and again.

(b) There are numerous indications that in the psychotherapeutic endeavour, knowledge cannot be built up on a purely cognitive basis. In order to support this point, I will give examples of bodily attunement, mood and the so called projective identification serving as vehicles to acquire knowledge within the psychotherapeutic process.

A clear example of *bodily attunement* is the following report by a therapist (Kruger, 1988(b): 68):

> A married woman, suffering from severe depressive states coupled with a strong tendency to drug addiction and also certain other states such as insomnia, anxiety and especially migraine, frequently took up much of the session with a tirade against her husband, because he did not love her, favoured his eldest daughter above the client, etc. At one session when she was especially worked up about this, I was determined to have her clarify her relationship to her husband. However, she interrupted herself to mention that she had had a terrific attack of migraine the previous night. Somewhat impulsively and imperiously, I suggested that we talk about that later and that we must now pursue the subject of her relationship with her husband. As soon as I said that, I suddenly felt intense pain shooting up my head which was most extraordinary because I practically never develop headaches, not even under fairly severe stress, and I certainly had never felt that particular type of ache in my head before.
>
> I immediately told her of this and she mentioned that these pains were similar to her migraine attack. It took us quite a few sessions to work out that the migraine attacks were a vengeful but masochistic way of living in total isolation of the family in which she had been scapegoated as a drug addict (she had in fact been hospitalised for drug addiction), and as incapable of being a proper mother. By refusing to look at the migraine within the texture of her family relationships, I had failed to see an intimate connection. I had isolated myself from the full quality of the relatedness and the need for powerful drugs to escape unbearably painful emotions and the pains in my head were there to remind me that I had lost contact with what was going on in the existence of my client.

In another reported case (Kruger, 1988: 181) an eminent therapist was required to see a nurse who was suffering from psoriasis. The therapist was able to come to a fairly clear conclusion that there was a strong psychological element in the psoriasis and "that her skin was a barometer of her unspoken feelings and the flare-up had enabled her to withdraw from a painful situation". The therapist,in coming to an understanding in this case, felt that she had been empathic and specifically mentioned that her *throat* felt sore when her patient left. Please note that she brings her sore throat into juxtaposition with her interpretation. The lived reality of her understanding of this client was as much based on her throat being sore as on her intelligent grasp of the life historical data.

Mood

Although I am convinced that a wide variety of therapist moods can serve as disclosive avenues, I am sure that the most frequent ones would be anxiety and boredom. As far as boredom is concerned, I do not want to give a specific example but I am pretty sure most therapists would understand that their boredom in many cases gave them an indication that the subject was trying to avoid the real issues, trying to talk without saying anything in particular, or simply sitting there in sullen silence. If the client is both dependent and needy I tend to feel drained after a session. As far as anxiety is concerned, an example is to be drawn from a therapist who reported (Kruger, 1988b: 186) that one of his clients came into the fifth session and,having said that she felt a failure, disclosed that she had painted three paintings, the implications of which were rather dysphoric, The first one was a painting of a foetal form in a chaotic world. In the second, she drew blood all over the page in a formless way, but when it began to take on a personal shape, she panicked and tore it into shreds. In the third she drew a map-like picture of a family in which she revealed a depth of rage and hatred that bewildered her and made her feel guilty. The therapist then reported on the thoughts he had in trying to understand and interpret what was happening to the client. Amongst others, he knew that she had terrible rages and he was somewhat tense about that. At that moment, he was feeling quite anxious and tense but whilst feeling the anxieties, suddenly realised what he had to say although it took him a couple of minutes to formulate a way of saying it. This therapist commented as follows:

> I do, however, remember feeling some of the anxiety as I suddenly "clicked" what she was saying. It was a couple of minutes, however, before I managed to formulate a way of saying it. In that time, I coped with my anxiety, realised that it would amount to quite a confrontation and might evoke a rebuke, contempt, anger (I didn't know which), decided that we had a good enough alliance to risk that, remembered the relief felt on previous occasions ...and formulated the right words. I also decided to take the pictures as a whole. I can't say my interpretation was "purely intuitive" or "merely an art", but it was something ... I should mention that by the time I spoke my anxiety had largely dissolved, and I was centrally aware of her pain.

From this, it is clear that the anxiety helped him to come to an understanding of what her rage and fear were all about. But obviously, the anxiety did not enable him to formulate his thoughts - this was a more cognitive process.

Projective identification

This is another set of examples where knowledge is acquired in a way which is by no means purely cognitive. Melanie Klein, as cited in J Sandler (1987: 15-16) initially saw this as a developmental mechanism and described it as a way of handling hatred in the infant against part of himself, as being redirected toward and against the mother. In this formulation the projective identification was, in the first place, an aspect of child development. However, in a second phase, countertransference was linked to projective identification. Thus the analyst was said to be made to feel guilty, sad or depressed as a result of the patient projecting something into him - the formulations used here are in typical dualistic language which most schools of psychoanalysis still seem to have to use.

In the third stage, projective identification is "described as if the externalization of parts of the self or of the internal object occur directly into the external object" (Sandler, 1987: 18). At the same time, it is pointed out by Kernberg (1987) that this differs from classical projection in that the client, rather than forgetting totally about what it was that was projected, still remains in empathy or in touch with what has now been projected "into" the "external object", i.e. the psychoanalyst.

My aim is obviously not to get involved in the exact nature of this psychoanalytic concept, but rather to indicate that, for my own purposes, the important point is that here we have a clear cut case of knowledge coming to pass in a non-cognitive way. My interest is therefore in how the psychoanalyst or, for that matter, any psychotherapist can come to understand how the phenomenon called projective identification manifests itself in the therapeutic situation.

In a case described by Otto Kernberg (1987) his rather disturbed (borderline) patient made some remarks about him indicating a rather condescending attitude towards him with reference to his (in her eyes) lack of intellectual depth, his "provincial" way of dressing, and his lack of a firm sense of self-assurance that his client liked in the men that she got involved with. Kernberg then describes how he felt. He notes (*op cit*: 106) that as the client "continued talking, I experienced a sense of futility and rejection". He thought about the fact that this client had been in therapy with quite a few therapists and that some of her previous therapists had conveyed to him that she was incapable of committing herself to a therapeutic relationship. He now thought that this was probably the beginning of the end of her therapy and he felt like giving up because he thought that he would not really be able to go beyond the well organised surface layer of the patient's comments. He further found that he was having difficulties in thinking precisely and deeply, exactly as the patient had just said. He also felt physically awkward and experienced empathy with the fellow with whom this particular client had just had an affair, and about whom she had spoken with derisive comments as to his sexual performance.

The analyst became aware in the final part of the session that he had been made into just one more of the devalued men whom she had first idealized and then rapidly devalued. There was a need on her part to assert her superiority over him and devalue him. He also realised that he was feeling much the way that she had described herself as feeling when she felt inferior and in despair - stupid, uneducated, incapable of living up to the expectations of the brilliant men that she had been involved with in the past. Kernberg also recognised in her behaviour towards him, the attitude of quiet superiority and subtly disguised devaluation with which the mother, as the patient had described her, made fun of her because of the inappropriate nature of the men she selected for herself. The clinical history showed that the patient's mother had a very derogatory attitude toward the patient and was able to subtly devalue her and that the patient sometimes identified herself with this aspect of her mother's attitude toward her.

Kernberg (p. 108) was able to interpret the happenings of the session in that her image of him as "intellectually slow, awkward, and unattractive", stuck in an ugly town, was the image of herself when she felt criticised and attacked by her mother, particularly when her mother did not agree with her selection of men. Kernberg states: "Her attitude toward me had the quiet superiority, the surface friendliness, and yet subtle devaluation she so painfully experienced from her mother". Kernberg further interpreted that in activating an inversion of roles in the same way as her mother, she might also be frightened that he would be destroyed and that she might have to escape from town to avoid the painful disappointment and sense of loneliness that would come with the destruction of him as a valued therapist.

Far from the clear and distinct ideas which Descartes required for the truth, we see here that knowledge comes to pass after a mood of futility and dejection has been experienced and only gradually becomes clearer when the therapist has been able to articulate his experiences in terms of his previous encounters and understanding of his client, and some details of her life history. Essentially he came to understand that the mood was an interactional, rather than an intrapsychic state.

Kernberg further comments that "this case illustrates a typical activation of projective identification, including the projection of an intolerable aspect of herself, the behavioural induction of the corresponding internal attitude in me, the subtle control exerted by her derogatory dismissal and self-assertion that kept me temporarily imprisoned in this projected aspect of herself and the potential capacity for empathizing with what had been projected onto me, because at other points it so clearly corresponded to her self-representation".

I cannot accept this explanation, based as it is on an anthropology according to which psychotherapy is concerned with two encapsulated psyches where the patient projects what is inside herself into the inside of the therapist. The further idea is that this part of herself is now somehow inside the therapist where she is still able to empathize with the projected parts of herself. Instead, what I see here is a very perceptive description of the therapist's attunement with his client couched in artificial dualistic language. In other words, he was sharing her tortured world dominated by her derogatory mother in which she was still identified with her mother and was casting the therapist in the same role as the mother had often cast the client. He had encountered the pathological attitude of his client and was initially unable to express it or to distantiate himself. The fact that he was thus, in a sense paralyzed, must obviously refer to his feelings of hopelessness, in that he wanted to help the client get better, but at the same time felt that with this attitude toward him she would simply be acting out her pathology which would tend to break the relationship with the therapist as had happened so many times before in her history.

In looking back over my cases in the past, I realize that I must have had more than one person with whom this phenomenon, called "projective identification" by the psychoanalysts, could manifest itself. As an example: Lester, was a borderline personality and he had frequently said that his sexual fantasies were not at all reconcilable with the way he thought about sex where he was very much in intellectual sympathy with feminist writers. He also gave an indication how weird and even sadistic these fantasies were but he also made it quite clear that he was not really prepared to discuss the content in any sort of detail. I found that this made me feel paralyzed but not only in the usual sense of feeling nervous and unable to do anything. Rather I repeatedly found myself feeling extremely sleepy and on one occasion I actually nodded off for a few seconds. Eventually I decided to discuss my sleepiness with him and was able to convey to him that I felt that he was controlling me in this way.

Having put forward his pathology but at the same time intimating to me that he could not really face discussing these issues and so being unable to entrust them to me, I referred to his relationship with me in which he saw me as being very like his father. While he rationally knew that I was fully accepting of him, he still had this almost atavistic fear that at any moment I could start shouting at him, telling him what a bad character he was etc. After this, however, and over the next few sessions, he was able to bring me some dreams and also to talk about his fantasies in such a way that we could deal with them. During these sessions there was not a trace of sleepiness in me. During this period, he came to understand that in spite of his progressive feminist ideas on sex, it was OK to be a flesh and blood person. During this time he even became sexually involved with a young woman. However, in one of his weekly sessions a few weeks after this affair started, he told me that he was not quite happy with the way things were going. He now complained that he felt listless, that his old thinking patterns were no longer functioning, that his life had changed, but that he did not want the change to be sustained. He said he wanted to make sure that he did not forget dreams, but in fact, he did. The interaction became desultory and I felt myself going off to sleep again. I told Lester that I was again becoming sleepy and that it was clear that he was avoiding the "heavy" stuff. He agreed and said that he himself was sleepy, but a few minutes later he remembered a long dream in which he had initially visited a fish and chip shop. However, the dream only really started when he was leaving this. Next he came to another shop which used to be a music shop but was now, in the dream, a sleep shop, i.e. they only sold beds and sleeping bags and other material for sleeping in this particular shop. Afterwards he was able to talk more easily about the (somewhat weird) nature of his sexual experiences.

To me this is striking evidence of how bodily attunement in psychotherapy can come to pass. Not only does a bodily and mood state indicate a similar state of affairs in the world of the client, and within the shared space of the therapeutic encounter, but also helps him to recall a similar theme in his dreaming existence.

What psychoanalysis calls projective identification seems to me to be a mode of being a therapist in which the therapist is attuned to a set of powerful but unowned themes in the existence of the client; themes which have neither been articulated nor even reflected upon in that particular therapy. Not only is the therapist unthematically present to this unarticulated mode of being but he also powerfully expresses such a presencing bodily and emotionally during the therapeutic encounter.

(c) The question of truth in psychotherapy

One of the main messages of H-G Gadamer's work *Wahrheit und Methode* is that method as such does not lead us to the truth (Palmer,1969). On the other hand, scientific research and method go hand-in-hand and certainly phenomenological research in psychology does imply a certain approach which has implications for an appropriate method to be selected. I take the freedom to apply Gadamer's message to my own field of inquiry inasmuch as I believe that truth is not to be achieved by method as such but by *insight*. It is possible to use "valid" methods most rigorously and yet discover nothing worth while. To take a very well known example: Freud always looked upon psychoanalysis not only as a therapy but a way of research as well. However, his psychoanalytic therapy plus his observation of a child throwing and retrieving a wooden bobbin did not lead him to the discovery of the repetition compulsion. It was his understanding of and insight into the *significance* of what the child was doing as well as his experience with his patients that led him to the formulation of this principle. Similarly the main thrust of this paper is that the mood and bodily states of the therapist are expressive of what comes to pass in psychotherapy and our task is to articulate the significance of these states in such a way that it does not remain limited to the therapeutic situation but becomes available intersubjectively.

Instead of following the rational natural scientific principle of trying to isolate unambiguous and distinct ideas and variables we should rather try to see truth in human scientific discourse as reflecting the ambiguity of life, as occurring within a certain context and as requiring formulation in connotative as well as denotative language.

In order to use these insights we will have to develop an insightful, descriptive language which remains true to the phenomena as they appear. Instead of trying to manipulate the information and/or the subject we should rather look upon the subject as an individual co-operating in our research. Moreover when we are faced with the phenomenon we should allow the phenomenon the freedom to be what it is; in other words we should endeavour to let the phenomenon show itself in all its facets. We know that this ideal is impossible to attain totally in that by showing some of its facets the phenomenon always hides others. However since it is impossible to exhaust life data we should be able to reconcile ourselves to this.

In human scientific research it is necessary to give up manipulation of the phenomenon in favour of allowing this to show itself by an intimate communion with it, we should also give up the logical positivistic differentiation between the context of discovery and the context of justification. From the foregoing it should already be clear that insight into the nature of psychological life does not mean that we lay hold of a clear idea or a well delineated fact like the weight of a stone. A detextualised phenomenon seems to me no longer to be the phenomenon which we were looking at in the first place.

It is also quite clear that the way we practice science should be reformulated. I think in spite of ourselves we are still sometimes affected by the ideal that once we have fully researched a certain phenomenon we can formulate it into some sort of a law and use some sort of technique or rule of thumb which can be mechanically applied. I think nothing is more fatal for true psychological research than just that. No matter how well the phenomenon is known from our descriptions these can never be applied mechanically. For example, when one is dealing with an anxious client, it does not matter how much one already knows from the descriptions and previous understandings of anxiety, one will find that every encounter will have to be a creative one if one is to understand the client fully.

I have no doubt that much of our most valuable knowledge of psychology originally comes from psychotherapy with psychoanalysis as the great pioneer. Part of our problem with this sort of knowledge has always been that it is not fully inter-subjective and that we are more or less captivated and limited by the profundity of the great pioneers. There are certain safeguards that we can use in order to overcome the possible one-sidedness of descriptions emanating purely from psychotherapy, e.g. there are ways we can pick up whether the story has been smoothed over, look for distortions in the text and so on. This question calls for a fuller discussion than I can offer here but there are other ways in which these things can be researched in such a way that the bias of the therapist is overcome, ie. by having a third party do phenomenological or other experiential research using both the client and the therapist as subjects and co-researchers (Kruger, 1983).

In conclusion, to reject the correspondence theory of truth in favour of a communion with the phenomenon in order for it to show its multiple facets,to reject clear and distinct ideas, discrete variables and measurable facticities in favour of ambiguity, connotative language and being emotionally and bodily in touch with oneself in the process of understanding the other person, is to come close to Heidegger's conception of truth as revealing (Heidegger, 1949). In contrast to the manipulations required in order to attempt to come to unequivocal conclusions, this conception rather asks us not to disturb the unfolding phenomena but rather patiently allow them to emerge and then articulate them.

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26

There's method in our madness: A deconstruction of the paradigm problem through psychopathology

Sandy O'Dowd

Perspectives, settings and periods

We will be dealing with three perspectives on the field of psychopathology: psychiatric, psychoanalytic, and phenomenological. Each of these perspectives emerged in a very different setting, and flowered in a different epoch. The psychiatric "medical model" developed in European teaching hospitals such as Charcot's Salpetriere after about 1870, and consolidated in the early decades of this century. The setting of psychoanalysis was the private practitioner's consulting rooms, and its spread in the 1920's opened a new epoch in the treatment of psychopathology (Showalter, 1987). The 1970's again saw a shift of fundamental importance, with movements such as anti-psychiatry, family therapy, and social work confronting psychopathology outside of formal institutional settings, in the sphere of common human existence. In this context, the existential-phenomenological perspective comes into its own.

What emerges then, is a concordance or correspondence between certain institutional settings, the practices that are instituted there, and various theoretical views and perspectives. The institutional setting brings the practice into a certain relation with its "object", and this relation in turn affects the nature of the practice. Hospital medicine is not the same as private practice. Before the recent "open door" policy, psychiatric hospitals were custodial institutions, structures which located the problem of psychopathology physically, by incarceration. Accordingly, classical psychiatry takes as its object the patient as physical organism.

The consulting room is in no sense a site of incarceration: it is a setting for a consultation, which is a serious conversation. Hence the confidentiality, the quiet, and the comfortable chairs ... Psychoanalysis engages psychopathology

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through the medium of the word, and thus takes as its object the patient as signifier, as a *rebus,* whose significance must be interpreted. Once the bounds of incarceration and of formal consultation are broken, however, psychopathology emerges into everyday life. There is no longer a setting as such, and hence no firm distinction between "patient", "relative" and "helper". In different ways, anti-psychiatry, family therapy, and existential phenomenology locate the problem beyond the individual, in the social matrix itself. This gives us the patient as human existence.

Incommensurability and beyond

A problem now arises, for these three perspectives locate psychopathology in different places, and thus appear to be talking about different things. Psychiatry locates psychopathology in the physical organism, and in particular in the nervous system. Hence the method of pathological anatomy which seeks for each disorder a specific site or location in the body. This method was developed in medicine and first applied in psychiatry by the late nineteenth century pioneers such as Charcot and Kraepelin. It remains the basic approach in organic psychiatry; indeed we mean by an organic mental disorder a condition attributable to a definite physical abnormality.

However, Freud broke with organic psychiatry and established psychoanalysis precisely through his critique and abandonment of this method. He noticed that hysterical paralyses ignore anatomy, and concluded that they follow not a physical but a mental organization (Freud, 1893). More specifically, he reasoned that in hysteria it is the mental image or representation of the body that is disturbed. Hence the patient as signifier and the interpretative method.

Finally, from our third perspective psychopathology is not "in" the patient's body or mind at all but "out there" in human experience. Existential phenomenology stresses the patient's fractured experience of being-in-the-world; family therapy places the problem in disturbed family interactions, and critical psychiatry in the social context of the patient.

In sum, we have three candidates for the site or place of psychopathology: the body, the mind, and the social matrix. This poses a serious logical problem, because these are categories of different orders, and hence knowledge about them cannot be directly integrated the way in medicine one integrates knowledge about anatomy, histology and physiology. To use Feyerabend's (1976) term, our three perspectives are incommensurable: no common measure can be found to make them directly comparable. Hence there can be no rational criterion for choosing one in preference to the others.

This kind of problem has lead some to see psychology as an immature, pre-pradigmatic science, still awaiting the emergence of a theory which will command consensus. There is some validity in this view, depending on how exactly it is interpreted. What is not at all helpful in my view is to see the three perspectives in competition, so that one will emerge victorious over the others as the consensual paradigm. From a practical point of view it is quite clear that for the foreseeable future we will continue to use physical, psychotherapeutic and systemic or social interventions in the treatment of psychopathology. Indeed the concept of a multi-professional team, which is steadily gaining ground, institutionalizes a multi-track approach.

Hence the problem which has occupied the philosophers of science, of how to choose between contrasting perspectives or approaches, simply does not meet the practical needs of psychology. The focus on this problem got the philosophy of science bogged down in the mires of epistemological relativism, and as Mary Hesse has reminded us (in this volume), it is time to move beyond that. If we have learned anything from the debate about paradigms, it is that logic fails one when dealing with multi-paradigm problems. Hence the way forward for psychology which I now wish to examine does not depend on logical analysis. The spectre of incommensurability arose from asking the question "where is psychopathology?", to which we got seemingly clearly incompatible answers: "in the body", "in the mind" and "in society". Perhaps this was a premature question. Asking "where is psychopathology?" assumes that it is a form which occupies a definite place or location. To prepare more carefully, more cautiously for this question I suggest that we go back to a prior question, and examine the form itself. Taking psychopathology as a realm, a terrain on its own, let us ask what kind of divisions it has, how these are organized, what sort of graduations or dimensions it has. Perhaps this will give us a better idea of how to then place it on other, more familiar conceptual maps.

Dimensions of psychopathology: Severity

Having reframed the problem in this way, the hard boundaries between the three perspectives begin to dissolve. Thus psychiatry and psychoanalysis share an interest in degrees of severity of psychopathology. This concept originated in nineteenth century psychiatry, when psychopathology was generally seen as a unitary form, hereditary in origin, and following a progressive and degenerative course. Thus in the process of degeneration, a patient would pass through a secession of more and more severely disturbed states. This model was possibly motivated by the overwhelming proportion of patients in the psychiatric hospitals of the time who were suffering from tertiary syphilis. This condition is inheritable, and does indeed follow a progressive and degenerative course.

At the same time, this unitary view found a ready justification in the crude Darwinianism which dominated scientific culture in the late nineteenth century. This "social Darwinism" confused biological evolution and cultural history, and saw Europeans as more "evolved" than other races, and men also as more "evolved" than women. The insane were seen as genetically inferior individuals, who were predisposed to "regress" back down the hierarchy of human development, eventually reaching a "primitive", childlike state (Showalter, 1987).

However, when in the 1890's the specific aetiology of tertiary syphilis became clear, the unitary view was dealt a mortal blow. Shortly thereafter, Emil Kraepelin, the great German systematizer of psychopathology, distinguished between what he called "dementia praecox" (schizophrenia) and the manic-depressive or "circular" insanity. Moreover, he characterized only the former as progressive and degenerative, and noted the intermittent course of the manic-depressive condition. Thus a new vision emerged: in place of a single form which progresses through states of increasing severity there emerged a series of discrete disorders which can be ranked by degree of severity.

Here the boundary between psychiatry and psychoanalysis breaks down completely, because this conception of a hierarchy of discrete disorders ranked by severity was the point of departure for the developmental model of psychopathology in psychoanalysis. Freud reasoned that the mind is a hierarchy of strata or levels of representation laid down during successive stages of development in childhood. A fixation or developmental arrest at any level renders further development unstable, and predisposes the individual to a later regression or collapse of functioning down to the level of the original fixation. Thus he could account for the new psychiatric model by simply correlating the disorders ranked by severity with his developmental stages.

The first detailed exposition of this model was due to Karl Abraham (1924), drawing on Freud's contributions. Abraham took Freud's three libidinal stages (oral, anal and genital) and divided each into two, giving five stages before maturity, in terms of which he could place five major disorders: schizophrenia, manic-depressive psychosis, paranoia, obsessional neurosis and hysteria (Table 1).

LIBIDINAL STAGE		PSYCHOPATHOLOGY
Genital:	Later:	[Maturity]
	Earlier:	Hysteria
Anal:	Later:	Obsessional Neurosis
	Earlier:	Paranoia
Oral:	Later:	Manic-depressive Psychosis
	Earlier:	Schizophrenia

Table 1: The Freud-Abraham Developmental Psychopathology (Abraham, 1924)

This linear developmental model in fact served to save the social Darwinist vision from the breakup of the unitary model of psychopathology. In his psychohistorical works Freud argued that ontogeny recapitulates social history, so that "savages" remain childlike, and the children of "advanced" cultures must retrace the track of social evolution on the road to maturity (Freud, 1914; 1927). In consequence the model not only legitimates racism, but also motivates a prejudicial view of the mentally ill, who are aligned with the inferior races. It was against this kind of prejudice, a hangover of the imperial outlook of nineteenth century Europe, that the anti-psychiatry movement launched its critique.

Dimensions of psychopathology: Modes of relatedness

Leaving the social issues aside for the moment, there are obvious problems with the linear developmental model: it allows for too few disorders, and it is not clear that the assumption of only one disorder per stage can be justified. Thus there is no place for phobic neurosis, or for the milder states of anxiety, dysthymia and hypomania, and the personality disorders are excluded completely. With the psychoses, it is not clear that paranoia is a less regressed disorder than manic-depressive psychosis; both Freud (1911) and Klein (1946) grouped it with schizophrenia. Even within classical psychoanalysis important observations were made which pointed beyond this model. Thus Jung (1907) reported striking similarities in the clinical presentation of hysteria and acute schizophrenic states, and Abraham (1911) linked obsessional neurosis and depressive conditions. Both of these connections, which we will examine more closely in a moment, cut across the neurosis/psychosis distinction, and cannot be accommodated within the linear classical schema. Rather they anticipate the contemporary but still unclarified notion of spectra of disorders, as in the DSM's spectrum of affective disorders.

The solution to these problems has emerged from a quite different direction, related to the debates between psychoanalysis and its more socially-minded critics. By way of introduction, we can say that psychiatry with its focus on the body and psychoanalysis with its interest in the mind found common ground in the analysis of the individual viewed as a psychosomatic unity, and of the psychobiological process of maturation. Analyses of mind and society converge on a rather different conception, focussed on interpersonal relations.

The theme which emerges here was stated with singular clarity by the phenomenologist Gabriel Marcel when he said that "in its own intrinsic structure subjectivity is already, and in a most profound sense, genuinely intersubjective" (Marcel, 1952: 182). In psychoanalytic terms: if the mind is a hierarchy of levels of representation, then at each level one can distinguish between self-representations and representations of others. In other words, we have here a candidate for a second dimension of psychopathology, an interpersonal dimension which cuts across the developmental, dividing each developmental level between the poles of self and others.

So far we are still dealing with an "intra-psychic" structure, a structure of representations. For phenomenology, family therapy, systems theory and critical psychiatry, however, the interpersonal dimension means more than this: it means that psychopathology emerges in the fabric of social relations, in an inter-psychical realm. One way of bridging these two approaches is to look at how individuals suffering from mental distress attribute the blame or responsibility for their conditions. This question was investigated by Foulds (1976), who distinguished between the patterns of intropunitiveness (self-blame) and extrapunitiveness (blaming others). He found that this dimension is clearly orthogonal to the dimension of severity, and distinguished intropunitive and extrapunitive groups at four levels of severity (Table 2).

LEVEL	EXTRAPUNITIVE	INTROPUNITIVE
I	Hypomania	Anxiety Neurotic Depression
II	Hysteria	Phobia Obsessional Neurosis
III	Mania Paranoia	Psychotic Depression
IV	Schizophrenia	

Table 2: Foulds' Hierarchy (after Foulds, 1976)

Note that this model accounts directly for Abraham's (1911) "spectrum" of obsessional and depressive states. It is also capable of accounting for Jung's (1907) observation of a continuum of states between hysteria and schizophrenia. The fact that schizophrenia divides between the two groups points directly to the problem of so-called "schizo-affective" states. Note that DSM-III in fact acknowledges the required three levels of depression, distinguishing dysthymia (neurotic depression), major (psychotic) depression, and finally melancholia, the hallucinated or schizophreniform type. In empirical terms this two dimensional solution represents a distinct advance over that classical linear schema (O'Dowd, 1986).

To return to psychoanalysis, Fould's inter-personal distinction, between intropunitive and extrapunitive orientations, in fact correlates very closely with various distinctions made in psychoanalysis in intra-psychical terms. Thus Klein (1935, 1946) distinguished between persecutory and depressive anxieties in psychopathology, the former representing the ego's fear of being damaged by objects, and the latter the fear that objects will be damaged by the ego's own aggression. In a similar vein Fairbairn (1940, 1941) distinguished between schizoid conditions in which the central fear is loss of self, and depressive conditions which focus on loss of the other.

These distinctions correlate well with Foulds' results: linking Klein's depressive position to Abraham's depressive-obsessive spectrum gives one the core of Foulds intropunitive group; Fairbairn (1940, 1941) saw the schizoid position as underlying the schizophrenia-hysteria spectrum, and Klein (1946) added paranoia and mania in her formulation of a "paranoid-schizoid" position dominated by persecutory anxiety, which is clearly extrapunitive.

Here the boundary between psychoanalysis and the social perspective breaks down, for the intra-psychic structures on which psychoanalysis focusses are revealed as mental representations of inter-personal situations. What emerges is a conception of *two fundamental interpersonal orientations or modes of relatedness which give rise to different spectra of psychopathology.*

Intersubjective subjectivity

We started with three perspectives which appeared to be incommensurable. However, it is now clear that this does not mean that they have nothing in common; indeed there are distinct lines of convergence between psychiatry and psychoanalysis on the one hand, and psychoanalysis and the social perspectives on the other. Foulds' Hierarchy (Table 2) is an empirical model of psychopathology which takes account of both these convergences, of both the levels of severity and the modes of relatedness. This result is of great importance, from both the conceptual and methodological points of view. Conceptually it motivates the apparently paradoxical conclusion that the foundation of subjectivity is intersubjective. The libidinal positions or modes of relatedness are not primarily forms of psychopathology but on the contrary constitute two "pillars of mental stability" (Bianchedi et al, 1984: 393) which are deformed in psychopathology. From a simple empiricist perspective one could argue that learning to be human necessarily involves learning both about oneself and about others. However, the concept of modes of relatedness points to more than this, for the two modes are not simply constituted of "self-representations" and "other-representations" respectively. Rather both modes involve both self and others in different forms of relationship. Hence it is more accurate to speak of a "self-centred" relatedness in the schizoid position and an "other-centred" relatedness in the depressive (O'Dowd, 1986).

From a methodological point of view this deconstructs the Cartesian notion of a sharp and fundamental dichotomy between subject and object, self and other, reminding us that we come to know ourselves in interaction with others and others through their interaction with us. An even more profound lesson emerges as a consequence. We arrived at this model by looking for convergen-

411

ces between seemingly incompatible perspectives. It now appears that the multiplicity of perspectives is not a problem, a reflection of the immaturity of psychology, but rather a necessary reflection of the multifaceted nature of human existence and thus human knowledge. This general conclusion can be drawn in more specific terms in a number of areas of enquiry.

Towards a multi-perspectival psychology

Firstly, the spectrial solution deconstructs the hierarchical social prejudices of the old psychiatric-psychoanalytic compromise. Both Klein (1935, 1946) and Fairbairn (1940, 1941) originally conceived their positions in developmental terms, with the schizoid or paranoid-schizoid preceding the depressive. This was consistent with the hierarchical view, aligning the depressive position with "advanced" Western culture, and the schizoid with "primitive" culture. Melitta Schmideberg (1930), Klein's daughter, was the first to suggest that cultures should be psychoanalysed not in terms of the classical libidinal stages, but in terms of the positions; however, she retained the hierarchical outlook.

Only once the positions are reinterpreted in non-developmental terms is it possible to see the equal dignity of different cultures. Anthropologists will be interested to note that in the depressive position conscience manifests in the sense of guilt, whilst in the schizoid position shame is the more typical reaction, which tends to support the distinction between guilt and shame cultures.

Secondly, concerning human development, we must now envision the possibility that there is more than one road to maturity. Precisely this possibility has been broached by Carol Gilligan (1982) in her studies of moral development, which show that the stages depicted by Kohlberg (following Piaget) are typically valid for men but not for women. I would suggest that men, granted structural power by society, are thereby structurally situated in the depressive position, afraid that their power will damage and/or alienate others. In contrast women find themselves structurally less powerful, and afraid of loss of self--in the schizoid position. Gilligan's studies confirm this analysis, for she shows that the typical crisis in moral maturation for a woman involves overcoming a split in the self between an external, compliant and giving self, and an internal incommunicado self which contains her drive for self-actualization. Such a splitting of the self is typical of schizoid dynamics, as depicted by both Fairbairn (1940, 1941, 1943) and Winnicott (1965).

Finally, we return to the metatheoretical problem. It should by now be clear that the initial judgement of incommensurability was indeed premature. There are substantial, albeit partial, convergences between psychiatry and psychoanalysis and between psychoanalysis and the critical positions. However, to work with these, and to work through them to a multi-dimensional model requires tolerating both logical and methodological ambiguities, and referring again and again to experience for clarification.

More specifically, we can see the necessity for seeking knowledge in both the "objective", other-centred mode of natural science and in the "subjective", self-centred mode of phenomenology. This is not a trivial connection. The psychoanalytic theory of obsessional neurosis and depression was worked out by Freud in his late period, while he was engaged in formulating the classical linear psychopathology and the compromise with psychiatry. Deep insight into hysteria and the schizoid spectrum, however, had to wait for Fairbairn and Winnicott, and it is precisely through their work that psychoanalysis has entered into serious dialogue with phenomenology. Hence the two modes of relatedness can indeed be seen as constituting two different modes of knowing.

In a sense, then, we can agree with Kuhn and Feyerabend that scientific progress is not always a rational affair, but must stress that in the case of the human sciences this is entirely appropriate, because the subject-matter is far from rational. By a humbler process of looking for meaningful patterns and significant forms we can hope to unravel the "method in our madness". Having travelled some way along this road we can see that within psychology a multiplicity of perspectives is quite inevitable, because human development is a multifaceted process. Just as there is no single road to maturity for either individuals or cultures, so there is no single approach to truth.

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414

Issues of conceptualization in the study of government and law

27

Theoretical approaches to the study of state bureaucracies: Some preliminary distinctions

D. Atkinson

How does one make sense of what happens in a government department? My recent empirical work on South African government institutions, such as regional services councils, local government and state departments, has encountered intractable conceptual and methodological difficulties. This paper will discuss some of the most important conceptual distinctions which have helped me to classify phenomena as well as the numerous theoretical approaches available in the literature.

This paper will discuss bureaucratic institutions, where the word refers to explicitly and purposefully created organisations. Sports bodies, churches and government departments are institutions in this sense. (This definition can be distinguished from "institutions" as sets of generally accepted, often not explicitly articulated relationships and norms. Kinship systems are institutions in this latter sense).

The paper is also intended to go beyond the conventional Weberian approach to institutions, which regards them as rational-legal constructs, operating according to explicit rules, to which superiors and subordinates must comply, in an ordered and predictable way. Instead, numerous researchers have pointed out that, in practice, intra-institutional relationships are much more complex, and that they must be analysed within a social and cultural context. Officials' own attitudes and perceptions often affect their social activities; complex power relationships develop; and social ideologies intrude upon the ostensibly rational procedures prescribed for the institution. Furthermore, institutions are often internally muddled, working at cross purposes. It is quite possible that the participants have unclear and conflicting goals, and yet the institution may survive and develop a powerful momentum of its own.

In practice, it is very difficult to understand exactly what goes on in a bureaucracy, and why. Apart from practical difficulties, such as secrecy, there

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are numberous available approaches (both theoretical and methodological) which a researcher can employ.

This paper has three purposes. Firstly, it will present two important methodological distinctions:

(a) Intra- institutional vs. extra-institutional approaches; and
(b) Relatively "concrete" vs. relatively "abstract" levels of explanation.

Secondly, to demonstrate the applicability and implications of these distinctions, two entirely different visions or substantive theoretical understandings of bureaucracies will be presented. I will refer to them as "constellations of concepts". The first, and most elaborately developed, refers to those theories which put concepts of power, interests, ideology and conflict at the centre of the analysis of bureaucracies. The second constellation refers to theories based on concepts of co-operation, consensus, legitimacy, social order and commitment. Most researchers, and even theorists, do not make their fundamental assumptions explicit. However, I believe that, by clearly recognising which constellation underlies a specific approach, we can go some way towards clearing the conceptual undergrowth which exists at present, and ultimately find sensible ways to reconcile these different visions, where appropriate. Personally, I find it hard to believe that any organisation can exist on power relationships alone, or only on co-operation and consensus. (For a more extended discussion of conflict and consensus approaches to the study of institutions, see Burrell and Morgan, 1979).

The third section is a brief presentation of a practical application of all these methodological and conceptual distinctions. The importance of information and knowledge in bureaucratic contexts is virtually totally unexplored in South Africa. Information and knowledge are curious entities, because they can be intelligently explained in terms of either "constellation of concepts". Furthermore, the two methodological distinctions outlined above also have a direct bearing on our understanding of information dynamics.

The paper will not argue for the rightness of any specific theoretical or methodological approach to bureaucracies. On the contrary, it is simply intended as a guide or a map of the many existing possibilities, and ventures to suggest that all these approaches will contribute to our understanding of such multi-faceted entities as institutions.

Distinction one: Looking inward or outward?

The first distinction to make is the extent to which the activities of an institution and its members can be explained in terms of intra-institutional or extra-institutional factors. To what extent do institutions develop internal dynamics which are powerful enough to be regarded as causes for patterns of behaviour? Peter Blau, for example, has analysed the hidden political processes within organisations. He found that one source of power in an organisation is based on the ability to control relevant sources of uncertainty and scarcity (Blau 1977:170). In contrast extra-institutional relationships and forces, such as class interests, socialisation, the structures of language and ideology, or other social institutions can also, be held responsible for what bureaucrats do.

To translate this issue into the South African context: Are officials (of the Department of Constitutional Development and Planning, for example), "captives" of their Department's ethos, perceptions, intrigues, relationships of power, patterns of loyalty, and information processes? Or can they rather be understood as (typically) Afrikaans-speaking individuals, from certain kinds of authority structures in home, school and church, with a certain understanding of politics and social order, or shaped by their class interests? Are certain institutions (perhaps the security agencies) more insulated from outside pressures than other institutions? Furthermore, one's theoretical understanding of the state, its internal coherence and its relationship to certain classes, will also affect the decision to begin one's investigation by looking at the inside or the environment of institutions.

Distinction two: From "concrete" to "abstract" levels of explanation

Another problem I encountered in researching bureaucracies is the vast diversity of government institutions. Can meaningful generalisations be made which could refer to government departments, municipal bureaucracies, regional services councils and joint management centres? To what extent does one's entire conceptual apparatus have to be readjusted to make sense of each kind of institution?

For example, do methods of socialisation in these different institutions differ so greatly (especially between more "open" and more "closed" institutions) that we need totally different ways of theorising their relationship with broader society (e.g. the degree to which bureaucrats' activities can be explained with reference to class interests)? Do the class backgrounds of municipal councillors and JMC officials impact upon their priorities in the same ways?

One way of dealing with this issue is to differentiate the degree of abstraction with which we are approaching a certain phenomenon. *Theories operate at different levels of abstraction.* More concrete theories are those theories which treat the existence of facts as unproblematic. In these theories, facts are regarded as existing "for everyone", and not as theory-dependent. Depending on the kinds of things we want to explain, this approach is quite legitimate. The discipline of public administration, for example, provides many useful insights about the effects of security of tenure on officials' behaviour. Factors such as information-gathering procedures or recruitment practices can have significant effects on an organisation's activities.

I regard these phenomena as "concrete", referring to the researcher's attitude (often shared by ordinary social actors) that these facts' existence or non-existence is indisputable. At this concrete level, the degree of diversity between institutions is usually at its greatest, and each research institution can have its own particular personnel, recruitment, information-gathering and many other kinds of characteristics.

Such concrete explanations can be used to explain both intra- and extra-institutional causal relationships as differentiated in the previous section. In the intra-institutional sphere, discussions of organisational "pathologies", red tape, inefficiency and corruption can yield useful insights about the behaviour of officials. Concrete explanations can refer to either "objective" factors, such as organisational rules and finances, or to "subjective" factors, such as perceptions, fears and beliefs of officials. Both can be employed simultaneously, for example, to account for conspicuous cases of bureaucratic conservatism. The existence of both subjective and objective kinds of factors can be verified by documentary sources or by interviews.

Let us consider concrete explanations by using extra-institutional factors. Officials' affiliation to other institutions, e.g. churches, the Broederbond, or periodic service in the defence force, could have an effect on their perceptions or behaviour. Or, by comparing salary scales within government departments to those in the private sector, we may develop insights regarding recruitment (the willingness of black councillors to serve in the black local authorities may be explained in this way).

However, once we go beyond this concrete level, and employ more theory-laden concepts, the ontology of organisations becomes more problematic. This happens for two different reasons. Firstly, concepts such as "power", "interests", "ideology" and "legitimacy" have been described as "essentially contested", since they are inextricably tied to a given set of value-assumptions (Lukes, 1974: 26). Secondly, once causal mechanisms are spelt out, and we move beyond the concretely observed phenomena, the perspective of the researcher becomes more and more prominent. I refer to this process of increasing authorial intervention as a movement from the most concrete level (empirical observation) to a more abstract level ("realist" explanation).

However, once we move beyond empirical observation, our basic assumptions direct us along a certain track, which consists of a constellation of assumptions and concepts. In this process "we select from ... "booming, buzzing" reality; we establish boundaries, we ascribe limit" (Ackermann and Parsons, 1966: 25-26). The same authors poignantly wrote, "We exclude - and what we exclude haunts us at the walls we set up. We include - and what we include limps, wounded by amputation. And ... we must live with our wounded and our ghosts". This paper will explore some of the implications of this statement in researching bureaucracies.

In this paper, I will give two examples of possible constellations that may influence our approach to the study of bureaucracy. These are the "power/interest/conflict" constellation, and the "social order/commitment/legitimacy" constellation. Each of these sets of concepts can be used to explain intra- or extra-institutional causal mechanisms, or both. Each constellation can also be employed at a more concrete level of explanation, or at a more abstract one.

Constellation 1: Power, interests and conflict

When I began my empirical research on government bureaucracies, I assumed that the concepts of power and interests would self-evidently be the appropriate starting-point. After exploring this idea further, it became clear that power can be understood in several ways. The difference between concrete and abstract levels of explanation can be demonstrated by working through the constellation of concepts in which the concept of *power* is central.

At the most concrete level (i.e. reducible to the publicly evident behaviour and self-acknowledged intentions of social actors), actor A has power over actor B if A can get B to do what he/she would not otherwise do. In this case, *A and B are both aware of the power relationship*. Steven Lukes (1974: 12-14) calls this form of power "one-dimensional", since the conflict between the two parties is explicit and acknowledged. For example, the normal apparently co-operative relationship between local authorities and central government departments in South Africa can, on occasion, be described as a power relationship in this sense. In 1986, the Department of Constitutional Development and Planning simply prevented the East London municipality from becoming involved in the development of the nearby black township. The same Department is at present attempting to prevent Conservative Party municipalities from re-imposing segregation. The conflict of interests was overt.

The concept of power becomes more problematic *if one or both actors are not fully aware of the power relationship.* In Bachrach and Baratz' study of "non-decisions" (quoted in Lukes 1974), B might experience grievances but feels unable to raise them, because he/she is blocked by certain institutions with specific characteristics. This is called "two-dimensional" power. Institutional practices prevent the articulation of grievances, and hence there is no overt conflict. The members of the institution (i.e. A, the power-holder) may not be aware that they are collectively affecting B's expression of grievances. For example, it may be the case that certain local authorities have grievances or perceptions which they are too timid to mention to relevant provincial officials.

A third form of power-relationship exists if B is not fully aware of what is happening. This is called manipulation. Again, there is no overt, visible conflict. For example, is the central government using the new policy of payment for city councillors as a strategy to raise the pecuniary expectations of "coloured" and Indian management committee members, which in turn would strengthen the argument of the government in favour of well paid, but racially segregated autonomous municipalities? This kind of power is called "three-dimensional" power (Lukes 1974: 23), where A seduces B into a false conception of B's interests.

The concept of *ideology* becomes very important here: B's subjective understanding of his interests may be very different from his real interests. In terms of the previous example, certain social observers in South Africa argue that it is really in the interests of "coloured" management committee members to demand racially integrated municipalities, instead of promoting their subjective/false interests by means of well-paid but racist committees. They maintain therefore that the government (A) is manipulating management committees (B) into a false understanding of their interests.

Any putative three-dimensional power relationships can partly be verified by actually interviewing bureaucrats or other social actors, or by considering documentary evidence. However, this kind of power analysis must go beyond what people actually say: even if all parties explicitly deny power relationships, the researcher may feel that her portrayal is still accurate. This would depend on the broader explanatory approach which she is using: why would she have reason to suspect that local authorities have hidden grievances, or that central government actors want to manipulate the subjective perceptions of management committee members? Why should the latter's protesta- tions not be accepted at face value?

The essentially contested nature of concepts now becomes quite clear. The researcher's value judgements about what constitutes "real" interests (as opposed to mere subjective ones), the existence of ideology (as opposed to "truth"), and the operation of hidden power relationships and social tensions, all form part of certain *a priori* assumptions about society. Concepts such as power, interests and ideology form an interlocking constellation. The researcher adds his own conceptual prism to the empirically observable phenomena.

An even more radical claim is that a power relationship could exist *if neither actor is aware of a power relationship*. This is the kind of claim made by theorists such as Marx, Althusser and Foucault. It will here be termed "fourth-dimensional power". Marx's analysis of ownership patterns, combined with new concepts such as "value", "surplus-value", and "abstract labour-time", allow a totally new interpretation of capitalism. Beyond the overtly free exchange relationship between capitalist and worker, is an unequal ownership structure which enables the extraction of surplus value from the worker. Neither capitalist nor worker need be aware of this.

Althusser's notions of "practices" and "structures" (1979) also purport to account for economic, ideological, political and theoretical relationships beyond the perceptions of actors. Behind the appearance of individual identity of choice, are structures which "create" and exert power over individuals. Similarly, Foucault (1979) analysed the masking of power by using the concepts of "discipline" and "normalization" (the administrative control of individuals). Using these concepts, Foucault could claim that social-scientific disciplines, together with certain techniques of power, can be used to "discipline" individuals, by imposing criteria of "normality" on them.

Fourth-dimensional power could, conceivably, be used to analyse what happens in South African bureaucracies:

a) In terms of Marxist analysis, the current complex class divisions,together with the financial, labour and other resources required by the various types of capital, may explain why the new policy on urbanisation gradually evolved within various government departments.

b) In terms of Althusserian concepts, the structuring of ideological practices could account for the ideological insulation and isolation experienced by certain government departments. It would be interesting to trace, for example, which ideological assumptions formed the foundation of the erstwhile Department of Co-operation and Development's administration of black people.

c) In terms of Foucaultian concepts, it may be possible to explain the various organisational reshufflings at central, provincial and local level in terms of a "normalizing" impulse, which disciplines social units by segregating them into officially sanctioned racial categories, and then re-integrating them in ways which allow for detailed government control and surveillance. The racial segregation of local governments, and their subsequent representation on the Council for the Co-ordination of Local Government Affairs, under the auspices of the Department of Constitutional Development and Planning, can be interpreted as an example of this tendency.

Several important points can be made about this kind of explanation of power. Firstly, these power relationships are not recognised as such by the participants. Foucault has been quoted as saying, "People know what they do; they frequently know why they do what they do; but what they don't know is what they do, does" (Dreyfus and Rabinow, 1983: 187).

Secondly, fourth-dimensional power is dispersed. It is not exercised in a purely unilinear way, by action A on actor B. Instead, power is exercised upon the dominant as well as on the dominated; all individuals are held in thrall by the relationships in which they find themselves. These sorts of explanations can be regarded as "structural" in a broad sense: individuals are constituted by roles and relationships. This does not mean that these approaches are fully deterministic. The grid or network of relationships operates like a grammar, which conditions what can be uttered in a language but does not determine which actual utterances emerge, and when (Hoy, 1986: 142).

Thirdly, at this level of explanation, institutions simply cannot be analysed any more without situating them within the broader social fabric. *Extra-institutional explanations are now invariably used.* The class aspect, or structures, or disciplinary techniques which operate in a bureaucracy are rooted in broader social forces. The application of such concepts is so far-reaching that it raises profound questions about the nature of the society or even the epoch. In terms of the concepts and categories "invented" by such theorists, the phenomena, activities or characteristics of an institution become united with similar phenomena outside the institution, often in unexpected places. For example, class relations (Marx), techniques of discipline (Foucault) or structures of theory (Althusser), found within a bureaucracy, are now seen as interlocking and supporting constituents of discourse, discipline or structures found elsewhere.

It is at this point, that the definition of "institution" which we have been using (i.e. consciously and deliberately constructed organisations) can be linked to institutions in the broader sense (unarticulated sets of relationships or norms), such as kinship structures. It would be fascinating to explore possible similarities between patriarchal South African families, and the authoritarian, paternalistic ethos in state departments.

It is evident that the creative role of the researcher is stretched very far here. Explanations now truly become "abstractions": they are imaginatively created, artificial constructs, created in such a way that they have their own logical coherence. This process has implications for the nature of concepts, as well as the role of the researcher.

As far as the nature of the concepts is concerned, the ontological status of their referents is unclear. Do "value", "abstract labour-time", "bio-power" and "structures" really exist, or are they analytical devices? Since the concepts structure what we see, we cannot, without circularity, fully test the truth or falsity of these concepts by means of observation. One of the implications of this issue is the problematic nature of causality: can the value of commodities "cause" the prices of commodities, or is "value" simply a heuristic device? Can bio-power "cause" certain social activities?

The higher the level of abstraction, the more blurry becomes the division between fiction and non-fiction. In the words of Robert Nisbet, "We cannot take away from Tocqueville, Marx, Weber and the other sociologists the visions for which they are famous: visions of mass society, industrialism, bureaucracy and the like. But we live in ignorance if we do not see clearly these same visions, albeit stated differently, in the earlier writings of such minds as Burke, Blake, Carlyle, Balzac ..." (1976: 8). Just as in art, social knowledge falls into "styles", and both "exist most luminously in the motivations, drives, rhythms, and itches which lie behind creativeness in any realm, artistic or scientific" (1976: 4). This is not to say that imaginative concepts do not reveal "truth", but to acknowledge that the distinction between truth and falsehood is itself created and sustained by the concepts we create.

Concerning the nature of the researcher: At this level of abstraction, our concepts tell us as much about ourselves as about the object of study. Our concepts structure the empirical details *for us,* and give them new meaning. It is at this level of analysis that the old positivistic disjunction between observer and "observed reality" is finally overcome. The analyst at last finds himself or herself situated within the perspective which he or she has developed. The more abstract and imaginative the level of explanation, the more the researcher herself becomes part of the material analysed. For example, if we choose to analyse class relations, ideological structures, or normalizing discourses, we would be very arrogant to exclude our own modes of thinking from the material being investigated. The distinction between concrete and abstract levels of explanation depends on the degree to which the concepts of the researcher creatively structure his portrayal of reality.

The more the researcher wishes to transcend the awareness of the social actors he studies, the more abstract his approach becomes. However, we must remember that theoretical visions and abstract imaginative constructions can eventually impact on concrete lived reality, when these constructions become popularly accepted, and actually begin to influence people's motivations and actions. (Marxism is a good example of this). This may serve as a kind of test of the usefulness of abstract theories: only those abstract and creative visions which can help ordinary people make sense of their situation in a new and more meaningful way, will eventually be absorbed into our conscious experience.

Constellation 2: Social order/legitimacy/solidarity

The importance of the creative role of the analyst can be emphasised by briefly working through another constellation of concepts. Instead of using the concept of power as starting point, different sorts of questions can be asked about institution. How does one account for the commitment of individuals to their institutions, and for their loyalty, sacrifices and altruism? This approach to public bureaucracies is virtually unknown in serious analytical work about South African institutions, and yet these characteristics appear to be very common in government agencies.

A few brief ideas for analysing institutions in this way can be provided, again at different levels of abstraction. At the most concrete level, the team spirit and expressions of mutual trust in organisations can be documented. For example, the enthusiasm and commitment of many officials within the newly-established regional services councils cannot be doubted. Interviews with officials in the erstwhile administration boards often revealed the same kinds of commitment. But how can we account for this camaraderie?

At a fairly low level of abstraction, we could look for determining factors *of which social actors are aware*. For example, exchange theory postulates that institutions are formed by voluntary agents, in pursuit of rewards and the exchange of equal values (Wilson 1983: 20). These exchanges entail several basic agreements between the parties: the parties' right to exist is mutually acceptable; the parties agree to the mechanisms and procedures of exchange; and the parties must find the outcome basically satisfying. Exchange relations therefore help to cement social interaction and order.

However, more penetrating questions can be asked about the nature of exchange relationships. Why do social actors perceive each other's existence as legitimate? Why do they wish to pursue certain goals and not others, and why do actors' pursuit of their interests reinforce social order? Approaches such as symbolic interactionism analyse the shared meanings and symbols, which provide a coherent network of generally accepted roles. In fact, it is only through such networks that an individual becomes aware of himself. The construction of the self and the integration of the community are simultaneous processes, as the individual adopts the viewpoint of the "generalised other" both towards himself, and towards the group as a whole (Skidmore, 1975: 150).

Such theories could account for the development of a group ethos within government departments. It may explain how many South African government agencies, and especially those that supply services to culturally different sectors of the population, manage to remain so insulated from the perceptions and experiences of their clients. In other words, the activities of such institutions could be explained in terms of the degree of internal solidarity within the institutions, instead of a putative desire to exert power and promote contradictory interests.

At a very high level of abstraction, we encounter functionalist theories. These usually entail much imaginative input from the analyst, as notions of function are dependent upon the researcher's basic assumptions regarding a healthy social organism. One example of such an approach is Mary Douglas' *How institutions think*. She has developed concepts such as "thought collective", which in turn would produce a "thought style", or a set of collective representations (1986: 45). Such a thought style would determine what would count as knowledge, as well as conceptions of the common good. Social rituals produce and reinforce unintended beliefs, which in turn help to maintain those rituals. It is in this abstract, metaphoric sense that one can claim that a social institution can think or feel.

This abstract level of theorising is characterised by the same elements outlined in the previous section. Functional relationships are not recognised as such by social actors; all social actors are subjected to these relations; and extra-institutional explanations are invariably used. Furthermore, the ontological status of the referents of concepts becomes unclear, and the analyst's mode of thinking is reflected in the perspective which is employed.

Pulling the threads together: The analysis of information dynamics as an example of different levels of theorising

So far, two main distinctions have been made: the distinction between intra- and extra-institutional explanations, and between concrete and abstract levels of analysis. Two typical constellations of concepts have been presented, both of which can operate at concrete *or* at abstract levels, and both can be directed at intra-institutional *or* at extra-institutional kinds of explanation.

Phenomena in government institutions could be explained using any or several of these options. As an example, this section will briefly discuss the role of information production and flow in the context of a bureaucracy. Using constellation l, information could be analysed as part of power relationships. Certain questions could be asked, e.g. who produces information? What power does it confer on the producer? Who decides how and to whom information should be disseminated? These power relations could be studied at a very concrete level. For example, the research unit in an organisation surely has significant influence on the nature of issues researched and information produced, and the resultant perceptions created in the rest of the institution. However, information dynamics can also be analysed at a much higher level of abstraction, as part of the discourse employed by certain institutional experts, which inadvertently legitimises their power. Foucault's notion of the "normalising" effects of knowledge can be regarded as a heuristic device to demonstrate how power is exerted by imposing "normal" categories on society (Hoy, 1986: 129-130).

Using constellation 2, however, we achieve different insights about the role of information in bureaucracies. At a more concrete level, information could be analysed using the tools of exchange theory, which would trace how information is produced and exchanged for other organisational resources, or even for social commodities such as prestige. This in turn creates and sustains the internal social order of the institution. At a more abstract level, a functionalist analysis can show how the society's construction of information is part of its entire metaphysics, and this in turn sustains social solidarity, even in the face of overt conflict.

These various explanations of the role of information do not contradict one another. A full analysis of information in bureaucracies could include both constellations of concepts, bringing into play intra- and extra- institutional factors, at various levels of abstraction. However, the higher one's level of abstraction, the more engrossed the researcher becomes within a specific constellation of concepts. It is conceivable, ultimately, that the seemingly uncomplicated and unproblematic phenomenon of a mundane departmental report, with its uncontroversial quota of information, is actually a rich source of insights about the whole institution, and ultimately the whole society. The concrete item, in this case the departmental report, can be visualised as the centre of a wheel,from which radiate various possible constellations of concepts, which can be utilised to account for the existence and nature of that item. At the rim of the wheel, at the point of greatest creative abstraction, is the researcher, who eventually finds himself being part of the wheel itself.

Conclusion

I believe it is impossible to prescribe one correct theoretical approach to government institutions (or to any other institutions, for that matter). Institutions could be analysed at a fairly concrete level, the way the discipline of public administration does. Or our approach could be more abstract and imaginative. In fact, the most rewarding activity is precisely to develop as many possible approaches as possible, and then begin the task of exploring their linkages, as well as our own imagination.

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28

Rights-relativism in Afrikanerdom: Van Warmelo's sociologism

John Hund

I

In Afrikanerdom today there is a large and increasing section of enlightened intellectual opinion that sees the modern idea of human rights as progressive and would like to see it implanted in South Africa and brought to full fruition as quickly as possible. It comes as something of an embarrassment therefore when some stalwart Afrikaner intellectuals ridicule the "universality" of human rights and deny their applicability to the "special circumstances" of South Africa and Africa at large. These embarrassing views are often pushed out of sight and wished away rather than confronted.

By dragging such views out into the open I am therefore running the risk of offending progressive Afrikaner colleagues. But my way of thinking is that sceptical and even cynical views about justice and rights in South Africa are not only fantastically misguided - they are cancerous - clippers need to be forged out of philosophical material with which to snip away these malignant growths before they spread further. These views need to be brought out into the open so that they can be diagnosed and treated.

So the purpose of this paper is to attempt to sum up views that I have heard or seen expressed over the years which, when taken together as a whole, form a picture of rights and justice which I find unacceptable. This picture is not difficult to grasp, even though it is seldom discussed in public, but it is extremely difficult to knock down in any clear-cut, once-and-for-all way, and it has proved to be an extremely potent weapon in the hands of demagogues. I do not have any hope of being able to change the minds of the latter, but if a clear picture of the view in question can be drawn, and if, in addition, some philosophical considerations can be brought to bear upon the issues it raises, a way might be opened up for persuading those who still reason about rights and justice

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in South Africa that there are better options left open for the future than this harmful and misguided view allows for.

My task is made somewhat easier by the recent publication of a paper containing the views of Professor Paul van Warmelo, an Afrikaner lawyer who has attempted to sum up his philosophy of justice and rights as they have developed over a long course of teaching and presiding as Dean of the University of Pretoria School of Law. I do not think the views he presents are "philosophy", at least not as I understand that term, because in my opinion philosophy presupposes a more neutral and rigorous outlook than is found in Van Warmelo's paper (1988). Therefore I will refer to Van Warmelo's set of views as an ideology. But though it is an ideology and not a philosophy of rights and justice, it has some philosophical aspects and it raises some important philosophical issues that can be treated from a proper philosophical perspective.

Van Warmelo believes that rights and justice, and in particular "human rights", are empty political slogans that express nothing more than the wish of individuals and groups to dominate other individuals and groups by force and fraud. Perhaps the simplest way of understanding Van Warmelo's ideology of justice and rights is to consider a matter that, for him, constitutes the very core of his thought about these ideas - the application in South Africa of laws that are widely perceived as unjust and discriminatory. Van Warmelo's theory is a justification of the application of apartheid and security laws by the South African judiciary. This justification is quite unlike the more familiar and overt positivist justification that has been, by now, academically discredited by jurisprudential writers like John Dugard (1978), A S Mathews (1971) and the late Barend van Niekerk (1970). Van Warmelo's defence of the South African judiciary is less well-known and considerably more difficult to knock down than the doctrine of legal positivism in South Africa has proved to be, and it is considerably more dangerous. As far as I know it has never been discussed in the literature. No-one has ever attempted to succinctly formulate the view itself, although as I shall suggest below I believe it has a fairly strong following in the South African legal profession and government, nor has the view been publicly criticized or debated as such. It has just been pushed into the background, and allowed to wreak its havoc undetected. This view is worth considering, not just because it provides us with an interesting insight into an aspect of South African legal ideology that has so far gone undisclosed, but because it presents a much more formidable threat to the removal of unjust laws in South Africa, and the normalization of its legal system, than legal positivism ever did.

According to this theory, South African judges apply unjust laws *and are beyond reproach for so doing*, not just because the range for judicial choice is essentially limited, but because justice is an essentially contested sociological concept with a variable and relativistic content. As long as South African judges are applying the law in conformity with principles of justice as they have been accepted in South Africa, their actions are not open to criticism. The argument, simply put, is that if the "justice" of the South African state (and the group whose interests it represents) differs from the "justice" of those who are oppressed by the state, or from the "justice" of some outside state or group that wishes to dominate South Africa, that is to be expected according the Van Warmelo, because:

> Justice is almost always justice for a particular individual or group (1988: 168).
>
> self-interest is always a dominating factor, for the simple reason that everybody realizes (consciously or unconsciously) that if he or his group has the upperhand, he has to hold on to it for dear life, otherwise the justice of the other man will triumph and woe to the vanquished (1988: 168).
>
> Justice often means "just us" (1988: 168).

It might be argued that such views as these are highly idiosyncratic and belong to Van Warmelo alone, that they cannot possibly be attributed to any larger section of South African society that Van Warmelo's views can be said to represent. I do not believe this is so, and I will give reasons below for suggesting that these views are a potent force to be reckoned with in South African law and politics. For now I just want to assess this view itself. First of all let me establish beyond any reasonable doubt that Van Warmelo himself uses this sociological "relativity of justice" argument in order to rationalize the judicial application and enforcement of unjust laws in South Africa.

Van Warmelo believes that the only source of rights in a society is the law (the sources of which he recognizes as custom, legislation and judicial precedent). He is quite explicit about this:

> If you consider what my rights are, it is the law and it is, I am sorry to say, the positive law which lays down what those rights are (1988: 168).

But what happens when the rights laid down by positive law are unfairly distributed as the result of unjust laws? What then? Van Warmelo is prepared to admit that this raises a "painful question" and that it is up to each person to decide for herself whether to respect such laws or not. Of course, if one decides not to repect such laws then one must be prepared to pay the price of disobedience without complaint, says Van Warmelo. Van Warmelo will allow that politicians and legislators in the white South African Parliament can rightfully be criticized for enacting laws which individuals or groups perceive as unjust, but what Van Warmelo refuses to allow is criticism of the white South African bench. He fears that such criticism can:

> ... develop into a subtle and devious breakdown of the standing and respectability of the bench which I, personally, still consider to be the most valuable and honourable element of our legal system and indeed of our society (1988: 168-69).

For Van Warmelo "our" legal system, "our" social system and "our" justice mesh together to form one, big whole which Van Warmelo identifies as represented by the South African state. He is willing to admit that other groups may have different conceptions of justice than that of the South African state, but he sees these conceptions as always based on self-interest. There is no independent, neutral or objective standard against which the "justice" of the South African state, or any state, can be measured. According to Van Warmelo, "justice" is just an empty slogan that detractors of the South African order use to bring it down, and the South African judiciary is doing an admirable job of preventing that from happening.

We are now in a position to understand Van Warmelo's skepticism and even cynicism about human rights in South Africa and in general. Many critics of the South African legal order have used human rights as a yardstick against which to measure the performance of the South African judiciary. But human rights are nothing but a cynical illusion according to Van Warmelo:

> The Rule of Law, Human Rights and the Law of Nature are all ideologies in the sense that they are products of the minds of individuals or groups who have been able to find disciples to agree with and follow them. Such ideas have no counterpart in reality (1988: 167).

Human rights have no counterpart in reality. They are mere group projections, based on self-interest, which are imposed by stronger groups upon weaker ones. Human rights are variable group perceptions. There is nothing "universal" about human rights at all:

> The notion of the Rule of Law has changed with times and has varied from community to community. In the same way the idea of human rights has also changed with time and varied from group to group (1988: 162).

Since human rights are mere ideology with no counterpart in reality, and since perceptions of human rights are variable and change over time, Van Warmelo wants to argue that there are no fixed standards against which to measure the "human rights situation" of any country. The only standards are those which have gained acceptance in other societies, past or present, and so it is just a matter of pitting the standards of one society or group against those of another.

According to Van Warmelo there is nothing "universal" about human rights at all. According to his way of thinking, the Universal Declaration of Human Rights does not reflect any "universal" conception of human rights at all. It merely reflects the composition and prejudices of those who took part in the drafting and signing of that instrument in 1948. Human rights relativists will reinforce Van Warmelo's arguments by pointing to the recent emergence of regional Human Rights Commissions and Charters, such as the American and African (Banjul) Charters. The existence of regional human rights movements, they say, signals the break up of post-World War II consensus on human rights.

They will say that the proliferation of "new views" of human rights gives support to Van Warmelo's relativist sociological theory. Cynics might even argue that it is just a matter of time until South Africa attempts to set up its own South African Human Rights Commission, using the "justice" of the South African state as the standard against which to measure the human rights situations in other African states. Since, according to van Warmelo, justice and rights are nothing but the self-serving projections of groups, there is no reason to suppose that South Africa would hesitate to impose its own "justice" on the weaker frontline states and the artificially created "homelands" and land-locked African states that are subject to South African rule. If van Warmelo's thinking is right, there is no reason for doubting that any of these events might come about.

This mockery of justice has frightening consequences. What really needs asking is how the legitimacy of such a scenario might be gauged. It could only be gauged on the basis of some standards of justice and rights that are not themselves the power projections of yet other states and interests, otherwise there will be no way of countering the objection that is latent in Van Warmelo's statement that:

> Both the Soviet Union and the United States plead for justice as they see it. South Africa, being part of the Anglo-American world, is constrained to believe in the American way of life, with its consumer society and one-dimensional man. On the other side there is the ideology of totalitarian justice (1988: 167).

Van Warmelo sees human rights as an Anglo-American version of justice, which is nothing more than a self-serving projection of power by a dominating bloc. The only reason that critics of the South African legal order use human rights as their touchstone, according to Van Warmelo, is that:

> Anglo-American ideology is, at present, the dominating one (1988: 163).

According to Van Warmelo, human rights is nothing but an ideology that the Anglo-American world wishes to impose upon the South African government, much in the same way that the South African government, through its legal order, wishes to enforce its own apartheid justice upon all those who are subject to it. The South African courts, in Van Warmelo's view, are doing an admirable job of enforcing South African justice. They have taken the high road by refusing to be intimidated into changing course by the dominant Anglo-American ideology of human rights or the Soviet ideology of totalitarian justice. This helps us to understand Van Warmelo's statement that:

> I shudder to think what would happen if our judges, good, bad or indifferent, were to be replaced by our politician lawyers (1988: 169).

The "politician lawyers" referred to by Van Warmelo are those who would remove the South African system of justice and replace it with their own Anglo-American or Soviet versions of it. If that were to happen, then, as Van Warmelo says:

> The justice of the other man will triumph, and woe to the vanquished (1988: 188).

In other words, justice is nothing but a contest of wills, power politics with a fancy name. Justice ultimately comes from the barrel of a gun.

Just how widespread is this view? Is it merely an unrepresentative and idiosyncratic view of an eccentric thinker? If so then we shall have to take account of the fact that that eccentric thinker presided over the Deanship of the School of Law at the largest Afrikaans-speaking residential university in the Republic for many years, and that as an outspoken critic of South Africa's detractors over the years he must have won the admiration of many lawyers and politicians who once sat at his feet as students. There is no way of knowing for sure just how influential this view have been, but my feeling, based admittedly upon impressionistic evidence, is that it has a considerable following in South African political circles and in the legal profession, even though it is seldom discussed in public. I think it is something that every school child in South Africa who has reached the age of reason must be aware of. Consider the statement below, given out by a South African law-maker and former student of Van Warmelo's, in the context of a debate in Parliament about rights in South Africa:

> If problems arise in a country, the fundamental question is who has his finger on the trigger? (D.J. Nel, NP, Pretoria Central, House of Assembly Debates 4 Feb 1977, col. 841).

Consider another:

> Experience has taught us that a Bill of Rights is not worth the paper it is written on (G. Marais, NP, Waterkloof, cited by A.L. Boraine, PFP, Pinelands, House of Assembly Debates 15 Aug 1983, col. 11300).

Or another:

> In Africa it is universally accepted that might makes right (D.W. Watterson, NRP, Umbilo House of Assembly Debates 16 Aug 1983, col. 11326).

Here we have a rationale that is entirely different from, and more intimidating than, even though it is interconnected with, the doctrine of legal positivism that purports to absolve South African judges from responsibility for continuing to apply unjust laws in the Republic. Its fundamental premise is the social relativity of rights and justice. When power politics is added to this sociological relativism, some familiar and dangerous views emerge: "might makes right" (Hobbes), "justice is the will of the stronger" (Thrasymachus), "law and justice are coercion" (Austin, Marx, Machiavelli), "the only rights are those of positive law" (Austin), human rights are "nonsense on stilts" (Bentham), etc. If we are to deal with these views we must see them for what they are, objectively, and stop pretending that they will go away if we just close our eyes.

II

I have just identified two strands in Van Warmelo's ideology of justice and rights that raise some important philosophical issues that can be treated from a proper philosophical perspective. The first of these is the view that justice and rights and related moral phenomena are relativistic sociological phenomena and not timeless Platonic forms. The second is that these sociological phenomena are often used in self-serving ways as tools of power based on greed and self-interest. I now want to briefly consider some of the philosophical problems raised by this first strand of thought, which I will call "Van Warmelo's sociologism". In Anglo-American philosophy today "sociologism" is having a hey-day. Perhaps we can deepen our understanding of the philosophical problems raised by Van Warmelo's ideology if we look into the current philosophical manifestation of this approach to justice and rights.

First let us briefly consider the recent history of sociologism in Anglo-American thought. Relativist theories of justice and rights obtained a toe-hold on Anglo-American thought shortly after mid-20th century when Wittgenstein's sociological theory of meaning and knowledge caught hold of the philosophical imagination and began to spread. With the view that meaning and knowledge, and the foundations of knowledge, were only relativistic sociological constructions on the rise, the old Platonic view that notions like "justice" and "rights" were timeless forms began to crumble. Before, when "reduction" was the heart of the logical-empiricist programme, philosophers thought themselves able to make actual contributions to the foundations of rights by giving theories which "analyzed the meanings" of rights and by considering their resemblances to and differences from powers, claims, duties, liberties, privileges and other contiguous phenomena. Today, the move is to "deconstruct" the foundations of justice and rights by viewing them as mere sociological constructions with no actual counterparts in reality. Let's examine a few of the basic assumptions underlying this "constructivist" theory of justice and rights.

According to the philosophical theory that justice and rights are mere social constructions and not timeless forms, rights and justice are thought to be language-dependent phenomena that cannot exist apart from the language-games and forms of life that breath life into them and sustain them. The community or group is seen as the source of all authority for assertions about what justice and rights are. In other words, justice and rights are what society says they are and nothing else. Since there are many groups in modern society, and many societies in the modern world, there are correspondingly many different and often conflicting conceptions of justice and rights in the world, and there is no one, single, neutral or objective platform from which independent judgments can be given as to which of these competing conceptions are right. Sociologism, by explaining the existence of justice and rights by what society lets us say, rather than the latter by the former, amounts to saying that we understand all there is to understand about justice and rights when we grasp the forms of life in whose language-games these notions appear.

Now that we have the basis of this relativistic sociologism before us and some understanding of the general context of its provenance in contemporary philosophical thought, we may be able to see connections between it and van Warmelo's less sophisticated but essentially similar theory, so that problems and possible responses to these problems that can be given in philosophical discussions of relativism can be transferred over to a consideration of van Warmelo's ideology. This may help us to raise the level of discussion overall.

Van Warmelo is sceptical about the "universality" of justice and rights, even when there is apparent "convergence" of perceptions of what these might permit or require, as for example with the Universal Declaration of Human Rights after the creation of that document shortly before mid-20th century. He interprets such convergence of belief, not as reflecting any underlying "universality" of human rights, but as nothing more than the expression of or hope for agreement amongst people or states. But we can respond to this skepticism about human rights by arguing that a better explanation for such convergence of belief, where it exists, is that human rights are "world- guided", that is, guided by how things in the world actually are, and by arguments about justice and rights that try to discover, in some conception of fundamental human good, standards that must govern the basic rights and institutions of any decent civilization. Van Warmelo's "conventionalism" too quickly rejects the idea that there is any such independent world of universal human values that can be specified independently of local group perspectives of what they may be, and to which philosophical thought strives to be adequate.

Van Warmelo's ideology of justice and rights insists that justice and rights come down to nothing more substantial than local traditions, practices, codes and even prejudices that constitute and define the ideals of each moral community or group. Since justice and rights have no purchase outside of conventions of that sort, he argues that anyone who behaves as his community requires can have no grounds for regret. This is the cornerstone of his refusal to allow that South African judges can be properly criticized for applying race- and security-based laws in the Republic.

We can respond to this argument by saying that we reject van Warmelo's conventionalist view that disagreement about justice and rights only makes sense as disagreement about what conventions and traditions, properly understood, requires. When it is argued about how far the rights which any society must respect leave people free to follow the distinct traditions of their own society, the argument presupposes that such rights are not themselves drawn from mere conventions alone. Rational debate about justice and rights involves a commitment to an objective point of view. There is often conflict between human rights and a community's local sense of justice and cultural identity, but never between the independence of the two. This is exactly where van Warmelo's ideology of justice and rights jumps the track. He wants to conflate the two, so that talk of conflict between them is not possible. He thus makes the mistake of conflating the normal with the normative, or the way things are done with the way they *ought to be done*. He sees no difference between the two at all.

Van Warmelo's sociologism comes down to saying that the only way of justifying or criticizing a certain practice in a certain society is to appeal to "normal" standards of justice and rights, as they are found in that society or some other society (or even in some hypothetical society). What is conspicuously missing from this view is the constant sense of how, as in a Karoo landscape, the road leads on to something beyond the visible horizon. And what results is the sort of blind enthusiasm for local prejudice dressed up as "justice" that we are coming to see more and more of in South Africa today. The counter tendency is empty rationalist talk about abstract justice. These two tendencies conspire to set up a false dichotomy which supplants serious critical reflection about justice and rights in the country. When debate about human rights comes to consist in disputes based on the horns of this false dilemma, it comes down to a drama between those like van Warmelo who enter appeals to what is "normal" or traditional in South Africa, and those who criticize such appeals by invoking abstract principles of justice and rights without apparently feeling the necessity of explaining what they are talking about.

This is just the sort of thing that we have seen develop between writers like Michael Walzer and Ronald Dworkin, when the former argues in his *Spheres of justice* that a system of distributive justice in a given society can (and can only) be shown to be just or unjust by seeing what would be said about distributive justice by someone "learned in his own tradition", (1983a: 43), i.e, the tradition of his own group, and the latter then takes him to task for urging nothing more than blind conventionalism. The only way, according to Dworkin, to escape uncritical conventionalism is to understand what "abstract justice" requires (1983: 44). Walzer's reply to this is simply to say that there are no such "pre-ordained forms" (1983b: 45). How might one respond to a conventionalist like Walzer or van Warmelo without falling into the trap of empty rationalist talk?

I think there are a few responses that can be given, once what is in question is understood, and in concluding I will list some of them. I will leave it to others to work them out in more detail than I have been able to provide here.

First, I think that most people would agree that perspectives of justice and rights are after all human perspectives and not "absolute" perspectiveless conceptions. Few people would argue that the meaning of human rights is like the shape of a diamond, fixed and determinate forevermore, rather than like its value which is different from time to time, depending upon external circumstances. Secondly, I think that few people would argue that rights are so sharp-edged that they can cut right through all attempts to use them as self-serving political tools. Any serious thinking about rights will have to acknowledge the fact that such inquiries have meaning only within the context of a certain informing tradition. Having conceded this much to the conventionalist however, I think that most people would stop short of all-out relativism of the type subscribed to by people like Walzer and van Warmelo.

I think that most people would agree that human rights appeal a faculty possessed by all people, everywhere and at all times, and that in disputing about human rights some of mankind's deepest feelings and beliefs are being engaged and adjusted under the influence of human reason. Most people will insist that human rights are largely independent of the peculiarities of individual human observers and that they yield not only a description of the modern world, but also an explanation and a condition of its success. When we appraise actions and institutions from the perspective of human rights we do not speak merely from within our own local institutions. No, we think of human rights in a wider, more abstract way. Most people will say that there is something inside of people which society recognizes, and cannot help but recognize, when it makes rights bestowals, and they would deny that rights are just a matter of what society bestows, and nothing more.

Few people would suppose that general acceptance of a view like Van Warmelo's, that there are no objective foundations of human rights, could leave intrinsically human practices and institutions unaffected. Unless there can be some neutral, independent and universal viewpoint, taken from the standpoint of universal humanity, from which local practices and institutions can be criticized, no serious criticism can be given at all. Denials that there can be such a standpoint prevent all criticism. This is the cornerstone of van Warmelo's defence of the uncriticizability of the South African judiciary. Such denials that there can be such a viewpoint rest on the idea that prejudice, including racial prejudice and discrimination, is so deeply built into the thoughts and traditions of people that no amount of rational criticism can dislodge it. And this is a standpoint that is ultimately civilization-destroying. It is, therefore, a standpoint that civilized people everywhere have found, and will continue to find, unacceptable. This is the real ground of justice and rights.

This paper has had limited objectives. I have not tried to "refute" what I have called van Warmelo's sociologism nor do I think that I should succeed if I tried. All that I have tried to do is outline his ideology of rights and justice and suggest that it poses a threat to human rights in South Africa if, and only if, it has a fairly widespread following in political circles and in the legal profession. Van Warmelo's ideology is of more than academic interest alone. In my opinion it represents a way of viewing things that is frightening and dangerous. Thus I have tried to look at this ideology philosophically, so that we can begin the process of reasoning about it, so that it can be defeated, as it should be. The last decade has seen South African legal scholarship opened up to sociological and socio-legal research with some very fruitful results, yet there continues to be a wide gap between traditional legal scholarship and philosophical research in South Africa on problems that are of vital importance to both communities of scholars. Perhaps the times is ripe for lawyers and philosophers to pool their skills to achieve ends which transcend the limits of both disciplines.

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29

Paradigms in comparative law: A critique

T. P. van Reenen

Comparative law as a legal discipline can be said to have reached a cognitive as well as an epistemic impasse. The major prevailing paradigms (cf. Kuhn, 1970; Mouton and Marais, 1985: 145 ff for the use of this term), which will be discussed briefly below, have all thus far failed to provide a sound basis for acquiring valid knowledge through comparison. This can be mainly ascribed to the fact that these employ either concealed, non-articulated or implicit points of departure which influence and determine their eventual concrete cognitive results in radically divergent ways (cf. Mouton, 1984: 104-106; Botha, 1984: 107; Radnitzky, 1984: 1, ff; Wisdom, 1972/73: 62, ff).

It is my opinion that this state of affairs is due to the non-recognition of the methodic meaning and relevance of a fundamental, ontic concept of law. In this modest contribution I attempt to establish the critical importance of a valid transcendental theoretical legal concept as basis and starting point for cognitively fertile comparison of positive (concrete) legal figures (institutions) in various legal systems.

It is not my intention (nor my ability) to provide a complete picture of comparative legal studies past and present. I shall therefore go about my topic selectively and try to expose certain centrally important methods of legal comparison identified largely on their prominent research characteristics as pronounced in the universal discourse on comparative law (see Mouton and Marais, 1985: 3-17).

These identified methods will serve as focal points for establishing and classifying shared views of the concept and purpose of comparative law into different ideal types or paradigms (Mouton and Marais, 1985: 18, ff; 145, ff). Within each paradigm I shall attempt to expose and illucidate the influence of various epistemic elements and principles on the theoretical perspective of its adherents as well as the mechanisms by means of which this perspective is blurred or even denied. The last three, regarded as the dominant modern

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paradigms, will be discussed in an attempt to put the most recent tendencies into perspective. The first three may be rather old hat to the informed and are therefore not discussed in detail.

A. Paradigms in comparative law

In this exposition I have relied to a considerable extent on the views of Frankenberg (1985), with critical remarks of my own. The dominant paradigms in comparative legal thought may be summarized as follows:

1. Encyclopedic comparison

This paradigm strives at the comparative delineation of the legal systems of all communities, places and times. The sense in which the term "encyclopedic" is used here differs radically from the sense in which it is used below (on p. 12, ff). This paradigm was initially suggested by Leibniz' design for a *theatrum legale mundi* (Leibniz, 1748). Later it was put into scientific practice by Wigmore (1936). At present it finds its intellectual home in the *International encyclopedia of comparative law* (1975). These encyclopedic, analytical or doctrinal comparatists pursue a less ambitious, more superficial goal than most of their counterparts. They examine the positive law of each compared society as a body of (potentially) coherent precepts, and by borrowing (often indiscriminately) established "working rules", try to improve the domestic law by cross-cultural comparison of positive legal institutions (*Encyclopedia*, 1975, various volumes).

2. Constructive comparison

This line of thought, which is the only openly prescriptive approach to comparative law, ranges from Aristotle's rational speculation about the ideal constitution, based upon the comparison of the constitutions of the Greek poleis of his time, via Montesquieu (1748) to more recent attempts at updating, unifying and improving the international legal order by means of comparative legislation (Ancel, 1978: 259; Lambert, 1903; Zweigert and Kötz, 1984, vol 1: 44-45; Herzog, 1957: 339, ff; Pollock, 1903: 74, ff). The constructive comparatist, armed with a preconceived, idealist view of the law, relies primarily on the written sources of law in visualizing and conceptualizing the ideal state, constitution and society.

3. Comparative historical reconstruction

This scientific endeavour, which is largely outmoded now, was the enterprize of legal ethnologists, with Post (1876; 1894) as precursor and the more philosophically orientated school of historical jurisprudence (cf. Maine, 1861; Pollock, 1903). This movement is firmly rooted in the analytical positivist ideal and should not be confused with the German historical school initiated by Von Savigny. The aims of these scholars were to reveal through the investigation of the origins and developments of institutions, forms and categories of modern law the evolutionary principles of law, to give a detailed account of legal pluralism or to discover the "*richtiges Recht*" that satisfied human social needs at any given stage of cultural development. The historical reconstructivist contemplates positive (concrete) legal relations and institutions diachronically in order to determine the way in which the natural or universal history of law has evolved.

Both this and the previous paradigm are ambitious in their approach, *viz.* the establishing of an ideal legal order through the founding of an ideal (or at least good) society, be it by means of an abstract universal ideal or the inevitable culmination of socio-cultural development. Both are anthropocentric and employ a prior idea of human nature in their constructivist and reconstructivist efforts.

4. Juxtaposition-plus

This is the comparative method favoured, for obvious didactic reasons, by the majority of textbook authors. It is *strictu sensu* neither a paradigm nor an ideal type of thought for it draws eclectically from and cuts inter-methodically across the other approaches to comparative law. This paradigm has its essential point of departure in the idea that legal problems are universal and perennial. It reflects the adherence of its proponents to the educational tradition with its focus on the study of cases, statutes and legal doctrine. Their analytical approach to the last confirms the notion that legal reality can be abstracted from and grasped in the various legal texts. Their method is basically the juxtaposing of (exerpts from) cases, statutes and doctrinal treatises (Liebesny, 1981: 1-2; Barton *et al.*, 1983: xiv; Merryman and Clark, 1978: ix-xii; Schlesinger, 1970: 1; Von Mehren and Gordley, 1977; Glos, 1979; Murphy and Tanenhaus, 1977; Karst and Rosenn, 1975: vii). This material is then provided with various explanatory and interpretative additions ranging from brief introductory remarks via descriptive sketches of historical backgrounds or systemic contexts to a more detailed analysis of similarities and contrasts. Some authors can be singled out for their theoretical expositions (e.g. Barton *et al.*, 1983; Karst and Rosenn, 1975; Cappelletti and Cohen, 1979).

Usually juxtaposition-plus is aimed at enhancing a keener knowledge of domestic law and its inherent foreign elements (Schlesinger, 1950, 1970), to open up foreign legal horizons (Karst and Rosenn, 1975), and to provide better insight into public and private international law (Butler, 1980). At first glance this method seems, and purports, to provide nothing more than the "objective facts", leaving any evaluation thereof to the student or comparatist. Closer examination, however, produces quite a different picture.

The first problem is that the *comparanda*, *viz.* the historical or national contexts, legal systems, periods of legal history or areas of law to be compared, are selected by the comparatist subjectively, sometimes arbitrarily. There is nothing inherently wrong with this, but the implicit assumption of the textbook authors that the reality of legal cultures can be penetrated through texts and exerpts alone raises serious questions and criticism.

The further assumption that the law is a coherent body of precepts with clear internal structures, in the form of "contracts', "torts", "criminal law", etc., and external boundaries, in the form of "legal systems" or "legal cultures", makes almost anything comparable to anything. The selected context depends on the author's (subjective) area of competence, his preferred field of study, but, more importantly, his preconceptions about law and comparison. Generally the contexts contain the "major legal traditions" or "relevant legal systems" as objects of investigation. The background is almost invariably the dichotomy: "common law world"/"civil law world", with other systems thrown in here and there for purposes of contrast and variation - more often than not to emphasize some or other superior quality of either system (cf. Schlesinger, 1950: ix-xvi; Glendon, Gordon and Osakwe, 1982: xvii-xviii; Liebesny, 1981). The texts selected to represent a specific legal system or culture are also determined by the author's subjective preferences and implicit theory.

According to Frankenberg (1985: 431, ff) there are three major variations and combinations of juxtaposition-plus:

(i) The systematic approach, which starts with general characteristics and abstract concepts or with the institutional infra-structure of a "real" or ideal type of legal system, whence it proceeds to identify similarities or dissimilarities in the other sub-systems or areas of law within the relevant context. This approach appeals more to the "civil lawyer" and European scholar than his Anglo-American common law counterpart. (cf. David and Brierley, 1985: 1-29; Zweigert and Kötz, 1984: 1-10; Cappelletti and Cohen, 1979: vii-x). From this perspective the compared legal systems are classified into "families", "styles" or "traditions".

(ii) The casuistic approach with a "factual focus of presentation" (Schlesinger, 1970: xv, 410-429). This approach is aimed at illustrating the technique of conflict resolving by using randomly selected cases from different legal cultures and epochs.

(iii) The topical approach which is closely related to the casuistic method and which focuses on "the law in action" in cross-culturally selected legal problems (Barton *et al.*, 1983: xv-xvi; Karst and Rosenn, 1975: 1; Cappelletti and Cohen, 1979:3-5; Murphy and Tanenhaus, 1977: ix).

What is seriously wanting in almost all instances is a thorough evaluation of the materials used, and especially discussion and criticism of the criteria of comparison and the ever elusive "neutral" *tertium comparationis*. In fact, a neutral referent for comparison is abandoned in favour of the own domestic legal system and culture which provides the conceptual framework and basis for comparison (Murphy and Tanenhaus, 1977, *passim;* Kommers, 1980: 657; Lawson, 1953; Liebesny, 1981: 1-2, 10, 155, 345; Schlesinger, 1970: xvi, *passim;* Barton *et al.*, 1983: 1; cf., on the other hand, *The western idea of law,* 1983, *passim*).

The main line of critique is not levelled at the fact that the known, domestic legal system is used as a home base for further reconnaissance, but at the way in which the comparatist's own system is given uncritical priority as being authoritative and natural and at the way in which the "other systems" are represented as radically different, inferior or even antagonistic (especially in the case of Soviet law). This is in essence what Frankenberg (1985) calls the "ethnocentric mechanism of cognitive control" (p. 433).

Adding to the remarks at the beginning of this discussion, juxtaposition-plus as a constructivist, doctrinal method of comparative jurisprudence strives at identifying and, if possible, formulating the universal constants of law and the common core(s) of legal systems on the basis of general principles and precepts (Schlesinger, 1961). In this approach it does not come as a surprise therefore that a subjective concept of law prevails and is regarded as adequate to solve the central problems of life and society through a purportedly objective analytical method (Schlesinger, 1970: 35-37). Legal texts, coupled with comparative comments, serve as the *fons et origo* of all legal solutions. This is what Frankenberg (1985) calls the "phenomenon of legocentrism of the discourse", *viz.* the constant re-affirmation of a central notion of law in the avowed attempt to re-evaluate and re-imagine it (p. 433, 445). The term "legocentrism" should probably be used with apology to Derrida who, in his critique of Western philosophy, described it as being "logocentric" in its entirety (1977).

A last point of criticism: The adherents to this approach often conceal their real interests and (theoretical) perspectives under a cloak of so-called objective didactic concerns in representing the material. This brings about that the academic production of knowledge, i.e. the eventual cognitive results, are determined by (subjective) contingent factors and not by critical, questioning scientific logic (e.g. Von Mehren, 1957: xi; Von Mehren and Gordley, 1977: x; Cappelletti and Cohen, 1979: viii).

5. Comparative functionalism

The reigning modern paradigm, of which Zweigert and Kötz (1984) are at present the principal exponents, the rise of which was almost inevitable. Although it has become the dominant paradigm of comparative legal research only since the rise of *Interessenjurisprudenz* and sociological jurisprudence for which Von Jhering (1968) is commonly held responsible, its roots can in modern times be traced back to figures like Montesquieu (1748) and Maine (1861). This line of thought was greatly advanced by the American realist movement of the early twentieth century (cf. Pound, 1908: 354; Pound, 1951: 1; Pound, 1955: 70; *Rechtssoziologie,* 1978, *passim*).

Comparative legal functionalism is criticised by Frankenberg (1985: 434; cf. also Gordon, 1984: 57) as a vulgar version of sociological functionalism, a term introduced by Luhmann (1981; 1984). Although the two notions share certain basic theoretical assumptions, for instance that systems do exist and that they have environments to which they are structurally orientated and functionally related, they employ divergent methods. The basic approach is, however, problem-solution orientated (in contingent social situations). The hypothetical, experimental search for "other possibilities" or "functional equivalents" is aimed at reducing the complexity of (social) reality and is essentially comparative in outlook (cf. Kamba, 1974: 485, ff).

Concentrating hence on comparative legal functionalism, it can be said that it characteristically assumes that society constitutes and guarantees the environment for law, which is regarded as a sub-system of the social system. Generally, society is conceptualized as determining the law, but on the other hand, that "the law" on its part also directs and influences social development. Exponents of this paradigm are obsessed with a realistic analysis of the "living law" in its two basic forms: "law in the books" and the "law in action". The legal system in its broadest sense, including legal texts, institutions and norms, is seen as providing solutions to the problems, needs and interests of organized society.

Four principal variations within the broad parameters of this paradigm can be identified:

(a) Legal reactivism, focussing typically on "developed legal systems", emphasizes law reform as the adaptation of the legal system to the changing socio-economic environment on the premise that the law answers to social needs and interests (Zweigert and Kötz, 1984: 27-31; Kahn-Freund, 1965, *passim*).

(b) Legal activism which, on the other hand, accentuates the primary role of law in effecting change in society (Zweigert and Kötz, 1984: 27-31; Kahn-Freund, 1965).

(c) The "law and development movement", which is taken up with socio-legal activism (Merryman, 1977). This variation conceptualizes law and society as inter-dependent but separate entities or realities.

(d) The "inter-dependenists", who, in combining the active and reactive properties of law, stress increasing legal reforms as well as social modernization through law.

These variations are nothing more than strains of the notion of social engineering through law, however. Until now, Zweigert and Kötz (1984: 1-41) have presented the most systematic version of functionalist comparison, which they developed with regard to their "legal families"-approach (p. 57-380). They subsequently applied their method to selected positive legal figures (or institutions) in the realm of private law (Zweigert and Kötz, 1984, vol. 2; for a citique, cf. also Constantinesco, 1971, vol. 1: 54-68).

Suffice it to say that functionality, based upon a prior *praesumptio similitudinis*, is the pivotal methodic principle determining both the heuristic and verificatory elements of the enterprize (Lepaulle, 1922: 852; Zweigert, 1973: 735; Zweigert and Kötz, 1984: 31). In the process it is postulated as the sublime *tertium comparationis* (Zweigert and Kötz, 1984: 36-37).

Transposing the legal system as a social sub-system, and thus assuming as the essence of the law its conflict-resolutory function, reveals the inevitable prior notion of law which provides the theoretical framework within which the explanation of a complex and confusing legal as well as social reality is sought.

A few significant problems that can be identified are, firstly, that comparison as a method presupposes knowledge of "the law" and "same function" to make hypothetical statements possible. Secondly, functionalists restrict comparison to grand, apparent (functional) similarities and exclude all fundamental differences. This in turn, however, presupposes knowledge of what is "same" and what is "different". Partly as a result of this, they are unable to solve the problem that apparently similar social and economic conditions produce radically different legal solutions or even no solutions, in the understood sense, at all. The lack of this functional (/causal) relation forces comparatists to reduce the explanatory, and therefore cognitive, claims of their theory or simply to deny all the non-technical qualities of the law. This tendency towards higher and higher abstraction eventually strips the concept of law of its normative meaning and contextual understanding.

Thirdly, functionalists hold forth a questionable evolutionary vision of legal development which entails either legal adaptation to social needs and interests or social manipulation and modernization through law. This symbiotic process is in itself highly contradictory and its validity not beyond doubt. Variables with regard to the law are analysed and formalized to fit into the grand evolutionary pattern (cf. Popper, 1972: 32, ff). This, ironically, denies the very basic premise of comparative functionalism, *viz.* differentiation. The only point it eventually succeeds in making is the senselessness of separating the law from its social, and other, context(s).

Fourthly, what started out as an ambitious project with high legal scientific aspirations and claims, eventually turns out to be no more than an affirmation of yet another stagnant, barren form of legal formalism. This method, which is essentially empirico-inductive in approach and which relies exclusively on positivised empirical legal figures (or institutions) *viz.* legal texts, institutions and specialized legal agencies, can be criticized as being "legocentric" too (Frankenberg, 1985: 438).

Fifthly, functionalists claim to be neutral, value-free analysts who do not have to face hermeneutic or perspectival difficulties. They claim to produce objective knowledge by freeing themselves from the burden of domestic legal preconceptions and exposing themselves to the independent, universal, factual truths. In their rigorous pursuance of a purported purely scientific ideal they translate their version of legal formalism into universal problems with a view to creating a closed system of perennial legal solutions. How far they divert, in doing this, from an ideal posed by Rabel (1978: 85, 89), one of their claimed, famous, intellectual forbears, remains to be answered. The main difficulties arise, however, with the practical implementation of the postulated pragmatic methodic rules (Zweigert and Kötz, 1984: 25, ff).

By distilling all law into a universal typology of solutions, "function" is reified as a principle of concrete, positive reality and denied its modal-structural character, *viz.* of ordering legal reality in a very specific way (cf. Hommes, 1965, *passim*). The abstract, theoretical "general law" and "universal comparative legal science" is exposed as but another form of legal materialism (cf. Zweigert and Kötz, 1984: 39; Lepaulle, 1922: 852).

6. Critical comparisons

This paradigm, which is firmly rooted in a radical criticism of the epistemic principles of society and its determinants (see generally Geuss, 1981), was first articulated in so many words by Frankenberg (1985), and can in many ways be regarded as a strain of the Critical Legal Studies movement (see in general, Unger, 1983; Hutchinson and Monahan, 1984; White, 1984; Johnson, 1984). For comparative law to be a productive exercise, much more critical work free from "ethnocentric mechanisms of cognitive control" will have to be done (Frankenberg, 1985: 441; cf. also McDougal, 1952: 915, 926). To achieve this, comparatists basically have to do three things:

(i) Recant the analytical ideal of a neutral stance and the objective status of knowledge by admitting and recognizing the fact that they are, in some way or another, of necessity participant observers, which in turn compels them to approach their endeavours in a (much more unassuming) self-reflective and self-critical way. By doing this they will eventually be freed from prevailing ideological epistemic constraints and subsequently attain true cognitive enlightenment and liberation (Geuss, 1981: 55, ff; Balkin, 1987: 765).

(ii) Admit the epistemic implications of culture and radically re-evaluate the cognitive rôle of the domestic legal consciousness in attaining and preserving a universal legal perspective (Geuss, 1981: 90; Hutchinson and Monahan, 1984: 233).

(iii) Make "hermeneutics" an integral part of their discourse, which implies a "deconstructive" approach to their material (Balkin, 1987: 764, ff; Culler, 1983: 89-110; Geuss, 1981: 55, ff: 76, ff) in an attempt to reveal contingent and marginal factors which could have a bearing on perspective (cf. Frug, 1984: 1277) and proceed from there to the "real comparative stuff".

Once the above-mentioned have been accomplished, it is argued, comparatists "... will be able to disengage from objectivism and move beyond the dichotomy between relativism and universalism to a critique of positivist models of knowledge and rationality" (Frankenberg, 1985: 444-445; cf. Bernstein, 1983). It is the notion of legocentrism that dichotomizes legal and social reality and practice and in the process grants law a realm, in the form of a reality, logic and language, of its own. It was the great merit of legal realism, and to a lesser extent of sociological jurisprudence, to have questioned this notion and to have paved the way for the ultimate radicalization of its views by the critical legal scholars. The latter, in their turn, were able to reveal and criticize the underlying false ideologies and epistemic premises governing the cognitive enterprizes and processes (Gordon, 1982: 281; Trubeck, 1984: 575; Balkin, 1987: 755). In this way, it is claimed, they succeeded in demystifying the law and revealing it as a feature of human rationality which forms an integral part of human inter-relations, communication and discourse, which, contrary to the "objective truth"-claims of legocentrism, admits to the ubiquitousness, amorphousness and ambiguity of the legal phenomenon (cf. Thompson, 1978: 86) which can and should be traced wherever possible in texts, institutions, actions, ideas and fantasies.

The task of critical comparative legal studies would then, in my view, be to initially, by way of historical analysis and explanation, expose the various intersecting social processes which shape(d) the comparative legal mind and (false ideological) consciousness(-es) and ultimately reveal a form of "positive ideological consciousness" which would further the real interests of the agents, *in casu* comparative lawyers everywhere, adhering to it (see Balkin, 1987: 751, ff; Geuss, 1981: 22, ff;: 45, ff).

B. A critique

Due to limited space the mentioned paradigms cannot be subjected to a thorough critique here. Hence, what I have to offer from my point of view should not be regarded as more than a few cursory remarks. The main points of critique to my mind are the following:

All the paradigms discussed employ a reductionistic, or even heteroductionistic, view of reality. These terms can be briefly illucidated as follows: Moving on the plane of the modal dimension of temporal reality, all concrete (positive) entities, in our case the positive law in its broadest sense, can be said to function or exist in a certain qualified way in reality. According to the philosophy of the cosmonomic idea (Dooyeweerd, 1984)there are fifteen such modes of existence or functioning inherent to all entities, e.g. the arithmetical, the physical, the kinematical, the biological, the social, the ethical, the jural, etc. When an entire entity is regarded as being primarily one of his attributes or aspects, in other words if the essence of a thing is ultimately explained in terms of only one or a few of its essential characteristics, the entire thing is reduced to that one characteristic or function (reduction) or a few selected characteristics or functions (heteroduction). If this is the case, one will never be able to (attempt to) a true (or approximately true) concept of anything.

Translated to the present context, this implies that, unless one employs an approximately true concept of law, you will never by approximation know the law and hence, never know what to look for in comparison. The eventual and inevitable consequence of this is that all comparison will be senseless since it will not produce any new, approximately true, knowledge.

This does not mean that none of the mentioned paradigms have anything positive to contribute to the epistemological parameters of the paradigm we are trying to establish. Far from it. The point made here is that the fundamental concept of law, which I regard as the *tertium comparationis* of all comparison, must in all contexts be subjected to continual critical appraisal.

1. Starting with the encyclopedists, the criteria they employ for classification are inherently contradictory. These are, *inter alia,* historical, geographical, religious, ideological, socio-economical and socio-political (see e.g. David and Brierley, 1985; Van Zyl, 1981). Nowhere a juridical norm is used for classification, which is indicative of an a-juridical view of legal reality in terms of which the law is reduced to whatever classificatory norm is employed.

2. In the case of functionalism, and to large extent of critical comparisons, the notion of law is reduced to an instrumentalist, pragmatic measure for social coercion and control. The dynamic nature and qualities of the law are overemphasized to such an extent that sight is completely lost of the ontic (or cosmic) constants of that same law without which no dynamics and change can even be imagined (Strauss, 1978: 15, ff; Hommes, 1983: 140, ff). The law is seen

as being entirely qualified by its social analogies (Strauss, 1978: 25, ff, 63, ff; Hommes, 1983: 403, ff). I am not denying the social functions of the law, but this line of argument denies not only the true fundamental nature of the law and legal rules, but at the same time the true nature of society and social rules (cf. Strauss, 1978: 20). Legal sanctions and social sanctions are regarded as merely mutually supplementary, which is simply not true.

3. Juxtaposers, on the other hand, only have eyes for the semiotic or lingual qualities of the law (cf. Strauss, 1978: 19; Hommes, 1983: 375, ff). According to them the legal truth is written and embedded primarily in the symbolic signs by means of which it is communicated as well as the various divergent interpretations of those signs in various, diverging contexts. Outside of that there is no room for any other legal reality. In this way it is easy (and convenient) to give the law a life and reality of its own and then subsequently to criticize it for what we have rationally constructed it to be according to ourselves. This furthermore confirms the senselessness and barrenness of employing reductionistic legal concepts in comparing laws.

4. On another but similar niveau, the empirico-inductivist and *a priori*-deductivist approaches and ideals of the constructivist and reconstructivist efforts respectively, fail just as dismally as the other already-mentioned enterprizes. In the first instance, empirical legal figures (or institutions) already presuppose a general fundamental concept of law and can therefore not be employed to construct such a concept by means of generalizing *per genus proximum et differentia specifica*. Secondly, deductivist methods will similarly not reveal the true characteristics of the law. Speculative idealism which postulates an ideal law from which all legal reality is then to be deduced, presupposes a basic concept of the law too. The ideal presupposes knowledge of the real (Hommes, 1983: 1, ff).

5. Only an attempt at theoretically disclosing the ontic (or cosmic) nature of the law and the encyclopedic analysis of the fundamental principles thereof, without which no valid legal concept can be formed (see Dooyeweerd, 1969, vols. 1 and 2; Dooyeweerd, 1984; Hommes, 1965), which in turn is embodied in positive, concrete legal figures (or institutions), will provide us with the essential *tertium comparationis*.

Of course comparative law eventually concerns itself essentially with the way in which a fundamental concept of law, which in my view could be regarded as justice, is embodied in the positive law in any given historical (and other) context (cf. Hommes, 1975), and not with the discovering of the ontic (or cosmic) essence of the law through comparison. It would be foolhardy, though, to deny any *Vorverständniss* in the form of pre-theoretical points of departure embedded in basic beliefs or *Weltanschauung* with which any given form of positive law is approached (Hommes, 1965, Mouton and Marais, 1985, Mouton, 1984, Botha, 1984).

Our experience of legal reality begins with the empirical, but it does not end there. Our sensory experience and observation is not the limit of our epistemic and cognitive encounter with that reality (Dengerink, 1980: 88-104; Strauss, 1979: 254-264). Unless the juridical, as an aspect of temporal (positive legal) reality, is encyclopedically contextualized in that reality, we will continue to work with a reductionistic and therefore attenuated view of that (legal) reality, and therefore the objectivity and perspective so sorely needed by comparative lawyers will always elude us.

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Understanding and explanation in anthropology

30

Understanding other societies through anthropology

P.A. Erasmus

1. Introduction

Anthropology is a product of Western science and anthropologists generally agree that the distinctiveness of their discipline derives largely from its study of non-Western societies, that is, other societies. In spite of the agreement on the emphasis of other societies, the interaction between the culture of the researched society and the researcher, is handled by anthropologists in different ways. In this regard at least two points of view can be distinguished for the purpose of this paper.[1]

Firstly, there is the identification, or otherwise, with the ideal of positivist objectivity. Here the view of *inter alia* the classic (unilinear) evolutionists and that of positivist orientated structuralism can be distinguished. In the case of the former, there have been attempts to eliminate all subjective factors when practising anthropology by keeping as much distance between the researcher and his research group as possible. This was done by having as little contact as possible with the research group. In general anthropological parlance, these anthropologists are referred to as armchair thinkers, and their method of gaining and interpreting knowledge is known as "distancing".

Lévi-Strauss, an exponent of positivist orientated structuralism, made it his aim to reveal the hidden structures of social and cultural phenomena by formulating "underlying laws" (Oosthuizen, 1987: 210). In order to realise this ambition for knowledge of other societies, he subscribed to the assumption that the researcher can find truth about the other society only if he can set aside all the presuppositions and perceptions that influence him in his own society. In contrast to the idea of distancing, the researcher is expected to disengage himself from his *own* society (detachment). Said Lévi-Strauss in 1973:

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> To come to accept oneself in others, the aim which anthropologists assign to knowledge of humanity, one must first refuse one's own self (quoted by Todorov, 1988: 4).

In approximately the mid-twentieth century, the positivist epistemology of the 19th century had to a great extent lost its impact on anthropology (Hastrup, 1987: 289). As a result of this, but especially because of the acceptance by anthropologists of the basic motives of idealistic and phenomenological philosophy, it was endeavoured to an ever increasing degree to recognise and establish the idea of human subjectivity as an indisputable priority in the process of gaining anthropological knowledge. Consequently anthropologists increasingly abandoned "distancing" in favour of "identifying with" (Frankel, 1987: 170). Although no general agreement exists on the framework from which "identification" should take place, the principle of cultural relativism represents for the majority of anthropologists the Archimedean point in terms of which the understanding of non-own societies should occur. Although the ideology of cultural relativism came under attack, there is at present, according to Keesing (1987: 174), an inclination among anthropologists to return to relativism.

Anthropologists have founded their understanding of other societies on epistemologically differing bases. The purpose of this paper is to discuss this matter further. Firstly, the main arguments of the above-mentioned epistemological viewpoints will be discussed.

Proceeding from the consideration that the authenticity of anthropological practice is to be found in the acceptance of the basic uniformity of man (cf. Bates and Rassam, 1983: xii; Garbarino, 1983: 95-98; Trigg, 1985: 76 and Todorov, 1988: 49), the view is taken that the understanding of other societies is also to be found in this acceptance. Following on this, the second part of the paper will reflect on the question as to what degree the idea of "detachment" or of "cultural relativism" enables the anthropologist to understand other societies.

2. The problem of understanding

Anthropological knowledge is often described as being hermeneutic in nature (cf. Frankel, 1987; Hatch, 1987 and Keesing, 1987). This means that knowledge is based on the anthropologist's interpretation of another socio-cultural text and the understanding thereof in terms of his own context. The translation of the hermeneutic problem (that is, the problem of understanding) in anthropology to a mainly methodological matter concerning interpretation (cf. Habermas, 1983: 225, as quoted by Trigg, 1985: 195), has on the one hand led to anthropology being erroneously placed amongst the so-called non-interpretive humanities and social sciences - as if there could be anything of the kind. On the other hand, this view has consequently neglected to obtain clarity on matters such as the dichotomy between society and the individual; the question of public and private experience; the question whether thought is empirical or aprioristic in nature (in other words, whether it is universal or unique to specific societies); the recognition of cultural relativism; etc. Obviously, within the scope of this paper (equal) attention cannot be given to all the subjects mentioned.

2.1 Basis of engendering knowledge

Various factors influenced the development of anthropology during the course of the nineteenth century. The rise of positivism at this time under the leadership of Saint-Simon and Comte and the publication of Darwin's *Origin of Species* in 1859 can, however, be listed as two of the primary factors.

The gist of nineteenth century positivism and of the unilinear evolutionist standpoints is well known. It is therefore not necessary to discuss fully the application of positivist orthodoxy by evolutionists here. It is sufficient to mention aspects such as: the emphasis on methodological naturalism, faith in the progressive advancement and emancipation of man, the purposeful effort of human reason to master the environment, etc. These are all components of positivism applied by evolutionists in their study of "primitive" societies, with a view to eventually understanding their own (that is, Western) society.

In order to engender context-free knowledge, these early anthropologists put a high premium on the application of the naturalistic view of objectivity. In practice it meant that they tried to keep the distance between themselves and their research subjects as great as possible. As a result of this, anthropology was soon typified as the study of societies and cultures by outsiders, the science that views societies and cultures from "outside".

According to Todorov (1988) "distancing" led to a superficial way of engendering knowledge, as a result of which the correctness of anthropological data, as well as its scientific quality, were questioned. In order to maintain the required distance, the anthropologist was, strictly speaking, also not supposed to take note of what fellow-scientists said or wrote because he could be influenced by it. As there can be no scientific growth or deepening under such circumstances, the survival of anthropology was itself at risk. Over the years, increasing discomfort about the postulates of positivist evolutionists developed among anthropologists. Consequently, anthropologists shifted their metatheoretical viewpoints to an idealistic and phenomenological basis, which led to an alternative method of studying man. The manner in which the field of study was conceptualized, for example, changed fundamentally. In this respect Hastrup (1987: 288-290) has shown how a clear distinction was made between the ontology of human reality and the reality of nature, and how the uniqueness of the intrinsic sense structure of typical human phenomena as against the otherness of natural phenomena was recognised. The epistemological irreconcilability of the various processes of knowledge acquisition and conceptualisation involved here was realised, and the necessary adjustments were made. On the one hand it is confirmed by the literature that a shift in emphasis occurred in the way data was collected - more weight was given to qualitative methodology. On the other hand, the criteria for judging the validity of findings, in so far as the self-reflection of the researcher in his frame of reference and scheme of interpretation was recognised as part of the verification process, were reconsidered.[2]

Hand in hand with the interpretive tradition, "identification with" was established as a basis for engendering knowledge in anthropology. In practice it meant, among other things, that the researcher lived for long periods among the research group in order to understand the views of those being researched, built up an image of the group through participating observation, and learnt the language. Simultaneously the results of this method substantiated the principle of relativism.

Opposed to, but also in conjunction with the above-mentioned two bases of engendering knowledge, a third one, that of Lévi-Strauss can be distinguished. For Lévi-Strauss, the practice of his subject is basically concerned with the unconscious universal structures underlying collective human thought and behaviour. He wants to expose and construct these structures so that cultural phenomena can be explained scientifically in terms of them. With this in mind, Lévi-Strauss combines the idea of "keeping a distance" and "identification with" in his standpoint on "detachment", by which he means that the researcher, when studying an alien society, must detach himself from the influence of his own culture so that the inner comprehension (i.e., the experienced understanding of the research group as well as that of the researcher) can be converted to an outer image. Elsewhere, with reference to the studying of other societies, he expressed this ideas as follow: "I do it with all the objectivity and indeed all the empathy of which I am capable" (Eribon, 1988:8).

Detachment is accomplished by means of the following four basic steps (cf. Todorov, 1988: 4):

* In order to develop a "liking" for the research group, the anthropologist must experience a kind of "maladjustment" towards his own society. Something must drive him to want to live outside his society and to do research. Because the researcher lacks an external point of comparison at this stage, the reason for his feeling of discontentment may not be clear to him.

* Subsequently the researcher involves himself with the alien society with a desire to understand it from inside. He lives among the "subjects" for long

periods, he learns the strange language and customs, takes part in activities etc. In spite of this, the researcher may still think and experience the reality within the research community in terms of his own concepts - he is unable to abandon them immediately. The difference between the researcher and the researched is not erased by the above-mentioned actions.

* The disillusionment when the researcher discovers that there is still a breach between him and his object of research causes him to mentally (and sometimes also physically) "return home". A kind of schizophrenia overcomes the researcher and he struggles with himself about the contradictions between the two sets of realities that confront him. Given time and patience the researcher may eventually succeed in understanding by means of comparison and compromise that a universality exists in human thought and behaviour.

* The last phase in the researcher's "detachment" begins when, on the one hand he no longer tries to understand these universal categories of thought and behaviour through his own reasoning or through that of the other society, and on the other hand when he succeeds in understanding these universal categories in terms of the determinants of the generally underlying system. In other words, in the way an "ordinary man" understands it. Because the anthropologist transcends the obviousness of his own habits under such circumstances, it is possible for him to study the other as well as his own society. For the same reason Lévi-Strauss accepts that it is possible for a non-Western anthropologist to do ethnographic research in a Western society and to obtain more or less the same results as the detached Western anthropologist.

Although Lévi-Strauss, as mentioned before, combines his standpoint on "detachment" with the idea of "keeping a distance" and "identifying with", he does not give equal weight to the basic principles that underline the above-mentioned methods of engendering knowledge - unilinear evolutionism and relativism. He illustrates his view by blaming relativism for the unwillingness among inhabitants of former colonies to be exposed to ethnographical research - according to him there is no question of antipathy to anthropology as such. In order to share in the advantages of the Western culture more quickly, the inhabitants of decolonized areas choose to think of themselves (according to the unilinear evolutionist idea) as provisionally undeveloped and not (according to relativism) as permanently different. Lévi-Strauss (although he does not agree with the evolutionistic approach of someone like Fraser) therefore prefers unilinear evolutionism to cultural relativism for ethical reasons.

The debate in anthropology about the basis of engendering knowledge was discussed briefly above. Summarising the foregoing discussion, it can be stated first that, as there exists a close connection between epistemology and methodology, the choice in favour of a specific basis of engendering knowledge has a particular methodological implication. Secondly, it is clear that upholding a universalistic or relativistic viewpoint in respect of the problem of understanding has certain ethical implications for the study of man. This subject is dealt with in the next section, where the place and role of the individual in regard to the understanding problem is given attention.

2.2 Individual experience within social context

At the outset it should be mentioned that although social scientists acknowledge that the "individual" and "society" represent phenomena of two different orders, it is usually accepted, on the grounds of the emphasis on the social nature of man, that social practice constitutes the essential context in terms of which individual acts are to be understood. Authors such as Marx (compare his well-known argument about how capitalism causes estrangement between *Unterbau* and *Uberbau* in man) and Wittgenstein (he uses the term "form of life" to indicate that terms and concepts are embedded in a particular social practice and cannot be understood independently of it) attempt to dispose of the dichotomy between society and the individual and the accompanying atomistic understanding of the individual as a source of knowledge. According to these authors "individual" and "society" cannot be separated and therefore they emphasize the idea of mutual dependency. Man's true self is revealed only in a social context.

If one accepts that man cannot be understood when detached from his social context, one should obtain clarity on society in order to understand the individual. In this respect it can be mentioned briefly that the degree of unanimity and agreement in the judgement of people often forms the basis for writers to distinguish between societies (cf. Thornton and Ramphele, 1988: 36 et seq.). Disagreements as a result of divergent views lead to the distinction of different societies.

Trigg (1985: 65) rejects this view. As individuals in the same society (can) usually differ drastically on fundamental issues, unanimity does not necessarily generate the centripetal power that holds societies together. The logic of this type of argument leads Trigg to conclude that when unanimity is used as a criterion to typify societies, it is possible, in its absence, to connect a single individual to the concept of society. Therefore the definition of society cannot be based on the premise of unanimity between individuals.

In contrast to this standpoint the acceptable criterion for Trigg is the identification and classification of societies on the basis of the perceptions of outsiders. External views can cause a group to perceive itself - although it need not be so initially - as distinguishable community in the *broad* society. In accordance with the we-them antithesis, Trigg accepts that the way in which other see "us" eventually determines "our" perception of ourselves. This in turn influences the way "we" understand ourselves. Although the sociological definition given by "them" of "us" need not be correct, it can have real results in respect of the structuring of a society.

If social forms can be classified into separate categories within the same society and structured accordingly on the basis of the perceptions of individuals, it follows that social institutions, customs, practices, etc. as such do not offer the only context for social explanations. Man's conceptual framework is therefore not determined entirely by society. As people belong to the same race and live in the same "objective" world, a genetically (besides a socially) determined uniformity at least is present among them. These common factors represent the gateway to understanding other societies and therefore it is important to understand the individual.

Authors have different viewpoints about the question of understanding the individual. Concurring with Durkheim,[3] Wittgenstein (1953) handles this matter by talking about the rules of a social form of life with reference to the rules of language games. The rules of a social grouping are responsible for the construction of particular social concepts and a social context. Man cannot understand the meaning of words without the necessary knowledge of the context in which they are used, and similarly the understanding of the actions of individuals requires the necessary knowledge (which arises from participation) of the social context in which it is embedded, and of the social concepts associated with it.

Winch (1958) agrees with this. He maintains that, as the participation of individuals in the activities of a social collectivity occurs according to its socially determined rules, understanding is based on participation. Participation according to laid-down rules does not necessarily imply (as it does for the behaviourists) that the participant fully understand the rules. Winch actually combines Weber's *Verstehen* approach with Wittgenstein's "taking into account of concepts". Pauw (1987: 84-85) points out that to Winch it is not the subjective mental condition of the person or group being studied that is important. He also does not assume that the researcher must be able to identify with the studied person's position and consequently duplicate his emotions, etc. and make them his own. However, he must try to understand from the "inside" - as in the argument that "you have to be religious in order to understand religion".

Apart from the controversy attached to Winch's viewpoint (does it after all, imply that an atheist cannot study religion) the possibility cannot be excluded that when a researcher is involved in studying a foreign society, he can assume that it is similar to his own. In such circumstances it can be expected that the researcher will interpret the rules and practices of the other society in terms of those of his own frame of reference, because he accepts that it fits in with it. The adherence to Clark Wissler's doctrine of cultural universalia emphasis this problem in anthropology. The anthropologist, for example, may have a specific concept of an institution such as marriage that does not necessarily correspond to the generally accepted concept in the alien society. Supported by the assumption that marriage occurs universally, he can now see his task as being merely to decide from this unverified concept which relationships between members of the opposite sex should be typified as marriage. The researcher's conclusion about marriage (and its universal occurrence) is therefore already contained implicitly in his premise.

There is also the possibility that anthropologists do not endorse the ontological assumptions that figure in another society. Consequently they cannot or will not take it into account and apply it in understanding such a society. Sotho participating in marriage ceremonies, for example, accept that their ancestral spirits give a specific sanction to the activities and thereby confirm the alliance between the two people concerned (and also their families). If a researcher does not acknowledge such a marriage ceremony because of his religious convictions, he cannot conclude in this case, in accordance with his metaphysical views, that a marriage has taken place.

Decisive answers to these and similar dilemmas in anthropology are mainly founded on epistemological, relativistic grounds. It is accepted that as concepts are relative to their socio-cultural frame of reference, external standards or criteria of truth and falseness cannot be applied.

In this discussion it was firstly pointed out that the social scientist understands the private experiences of the individual as seen against the background of the public context. Secondly, it was stressed that the emphasis placed on the public context should not lead to neglecting or ignoring the importance of the meaning of private experience and understanding. In this regard it was maintained that private understanding and experience reveal a basic uniformity that makes the understanding of other societies possible. Thirdly, reference was made to cultural relativism and the basis it offers anthropologists in understanding other societies. The contradiction between relativism and universalism has not been discussed and will be dealt with now.

2.3 Cultural relativism vis-à-vis the uniformity of man

In general it can be said that the doctrine of cultural relativism in anthropology is characterised by the predominant application of normative relativism, whilst recognition is given to a lesser extent to what Frankena (1973: 109) calls meta-ethical relativism. The discussion focuses mainly on the first viewpoint, which is supported by well-known anthropologists such as Boas, Herkovits and Benedict.[4] In reaction to the classical evolutionist monistic view of culture, these exponents assume a position against the idea of universalism. They plead for sensitivity in respect of cultural particularism, hold the view that opinion on matters such as truth and morality can only be understood, defended or justified in terms of the cultural frame of reference to which it applies, and

465

emphasise, with a view to restraining ethno- and Eurocentrism, the need for neutral objectivity (that is, an open and receptive mind) when studying culture.

When reflecting on understanding other societies in terms of the premises of cultural relativism, the most important question is whether an action can be taken out of its cultural context and be considered and understood other than the way the agent himself understands it. Put differently: Why is it important in respect of understanding other societies to take into account the fact that an action takes place in a specific cultural context?

To answer this question, the meaning of the concept of *culture* will have to be discussed briefly. According to Thornton (1988: 20 et seq.) the premises of the following schools of thought are contained in the concept of culture:

* Romanticism, where culture is seen as the organic product of a community, people or nation.

* Enlightenment, where culture is the rational practice which leads to the general improvement of man's life; all people have it and it confirms the uniqueness of humanity.

* Modernism: Although the unique association of a culture with a specific people, community or nation is emphasises as in the case of Romanticism, it is also claimed that as culture functions as a unit within the parameters of a community, etc., it is culture that determines and maintains a community.

* The recent viewpoint that culture should be regarded as a *resource*. Like any natural resource, it is accessible to all individuals or groups - admittedly not on an equal basis, because the environment restricts what may and what may not and that leads to cultural differences. However, the existence of cultural differences is not used to explain different customs, etc. but to illustrate their meaning, function and history.

The concept of cultural relativism developed out of the cultural views held by supporters of Romanticism and Modern Times. If man, as these exponents of the said two viewpoints allege, is enclosed in separate, culturally formed societies and is unable to see the world other than through his own cultural spectacles, it is also not possible for the anthropologists to detach himself from his own culture and to join a strange society and master its concepts - in other words to base knowledge on the premise of cultural relativism. Seen in this light, it would appear that the epistemological grounds on which cultural relativism is based, are not compatible with the concept of culture from which it arises. Even if a case could be made out that it is indeed possible for an anthropologists to free himself from the influence of his own society and to apply the principle of cultural relativism, membership of a culturally formed society is not the most important aspect of being a human (cf. Coertze, 1980: 139 et seq.). Whichever way the application of cultural relativism is argued according to these cultural views, there are consequences that are not logically realisable.

Proceeding from the previous remark, it should also be mentioned that the acceptance of the doctrine that each culture constitutes its own reality (supported by a culturally specific truth) and can be understood only in terms of that reality, implicates the scientific nature of anthropology.[5] As culturally specific truths confirm nothing more or less than that a bearer of a specific culture believe that certain things are true (cf. Dimen-Schein, 1977: 38), the concept of "truth" in this case is reduced to that which culture bearers accept as truth. Under such conditions the task of the anthropologist is degraded to reconstructing concepts and reflecting on them, and there would be no sense in studying an alien society in the first place.

In addition, it means that where corresponding customs exist in different cultures, each is caught up in its own reality by distinguishable cultural truths. Cross-cultural comparison (one of the methodological building blocks of the science) and the objective testing of competitive theories are not possible under such circumstances. For the relativist, reality is established by the intracultural perceptions of people, and therefore the rejection of a realistic standpoint about truth (in other words that it is indeed possible to evaluate competitive interpretations of reality in terms of general values, etc. - cf. Mouton, 1986: 7) is almost inevitable.

Taking a stand against cultural relativism not only requires arguments about the logical consequence of its application, but also requires the promotion of the opposite of the argument. In this respect a connection could be found with the opinions of authors such as Wundt on the *Völkerpsychologie,* or of Kant, that a single universal set of categories exists which forms the conceptual substructure for human thought. However, an illustration of Collin's (1985) viewpoint on human predicaments will be used as a starting-point in this discussion.

Collins does not consider human predicaments in terms of individual psychological abilities or socially constructed conceptual representations, but as a given situation that arises from the interaction of the human biological composition with its geophysical and technical circumstances. By this he does not mean that it is possible for an individual to penetrate the cognitive framework of another because the inherent conceptual schemes are identical, but that people are gifted with a common biological heritage and experience the same kind of problems in their existence. Trigg holds a similar opinion.

Because people are confronted by the same empirical reality, he talks about the existence of common human reactions to an objective world.

Although technological, economic and social circumstances and factors can give rise to predicaments that are not universal (in other words, that are specific with reference to particular times and places), there are basic predicaments that occur universally and as such define humanity. The body, for example, is an essential but not a sufficient condition for humanity. The physical must be complemented by a psychological identity - the latter arises mainly from and rests on social relationships. No specific psychological or social complementarity of a physical identity, however, can be regarded as essential to humanity. If the social complementarity involved in the identity of humanity does not imply a specific social identity, the possibility of a gap between "private" and "public", as argued above, must inevitably be recognised. Such recognition underlines the significance of private experience in the understanding of other societies.

A further implication of Collins's standpoint on basic universal predicaments that define humanity, is that thought did not originate historically and also does not vary transculturally. As thought is inextricably linked to "society" (in so far as the latter forms the context within which it gains meaning), it follows empirically from the fact of social experience.

Finally it is important to note that viewpoints such as those of Collins and Trigg definitely do not imply that institutions in different societies necessarily correspond. Because societies, for example, do not have the same access to resources (physical, cultural and otherwise), institutions will differ from each other. The existence of differences is consequently not denied. They try rather to understand and explain such differences on the basis of the occurrence of general human imperatives.

In the preceding section cultural relativism was discussed firstly and motivations were given as to why it does not furnish a satisfactory answer to understanding non-own societies. Secondly, the standpoint that the understanding of non-own societies is to be found in the uniformity of man was defended.

4. Conclusion

Léopold Senghor, when contemplating his views of a gradual symbiosis of all civilisations into an universal civilisation, asked the following question: What would Ancient Greece have been like without Egypt, and Egypt without the Nubian Negroes? Proceeding from this line of thought, universal, basic human qualities, desires, longings, psychological needs, tendencies, etc., in other words pan-humanity, were mentioned in the discussion. Therefore, regardless of whether the Westerner conveys his departed loved one to his last resting place in a black Cadillac or whether the Zulu takes the spirit of the deceased on the branch of a thorn tree to the home of his ancestors, the same consciousness characterises these actions. When the anthropologist does not give recognition to, find an affinity with or move from the individual to universal humanity, the thorn tree branch or the Cadilac, in a manner of speaking, becomes more important to him than the person behind it. Stated differently, when the anthropologist places too much emphasis on the strangeness and uniqueness of other societies and strips them of "humanity", it can result in an inability really to understand such societies.

Notes

1. For reasons of space and time the positivistic and interpretive traditions were selected for this paper. For the same reasons the views of the exponents of critical anthropology (although they treat the issue of understanding other societies in a particular way) are not discussed. Inspired by the contribution of Paolo Freire, critical anthropologists with their so-called action research made the subject of study a full participating partner in the research process. As a result of the latter's involvement in formulating the problem, data collection and producing feedback on the draft report, the relationship between researcher and subject of research is largely equalized.

2. Caplan (1988) considers that in practice the anthropologist is still inclined not to expose and/or examine himself in his research results. The standard monographs therefore do not reflect the researcher as a human being (with everything that it entails). After appearing briefly in the preface to give the credibility of "I was there" to the report, he immediately disappears from the rest of the text.

3. According to Collins (1985: 51), Durkheim's standpoint on the empirical origin of thought anticipates Wittgenstein's argument in this regard. In contrast to Kant's aprioristic point of view, Durkheim accepts that thoughts/concepts arise from experience. As collective experience is at issue, he rejects the methodological individualistic explanation of individual behaviour.

4. For the sake of completeness it should be mentioned that as far as meta-ethical relativism is concerned, a more tempered relativism is to be seen. Although it is still accepted that an evaluation cannot be made independently of a specific cultural frame of reference (in other words, that there is no generally valid, rational way according to which values, norms, customs, etc. from different cultures can be judged against each other), the presence of cross-cultural universalia or cultural invariants is recognised, provided that it can be confirmed and verified empirically (Renteln, 1988: 61). The possibility that

cultures overlap in certain areas and that specific communal values are established as a result of this is accepted, but subject to an empirical epistemology - theoretically therefore.

5. The limitation of truth to the borders of a culture is, according to Trigg (1985: 71), an intentional action on the part of anthropologists to safeguard them from any (unfavorable) assessment of value about other cultures. In this respect he talks about a kind of post-imperialistic sensitivity which inhibits anthropologists to such an extent that they fear that any opinion not disguised by relativistic subjectivity may be dismissed as arrogance and intolerance.

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31

Micro and macro levels of analysis: The "unit of study", a non-problem in anthropology

Kees van der Waal

1. Introduction

The *unit of study problem* tends to vary in importance between different kinds of anthropology. For some the units to be studied are self-evident, for others the study of social boundaries has become a major methodological problem. In the recent past the interaction of social units and the variability of social boundaries have become matters of explicit discussion in the social sciences in general, and in anthropology in particular. Despite much rethinking on these issues, conventional anthropological concepts and methods seem to have considerable tenacity. Their ability to outlive theoretical fashions and bandwagons is demonstrated by the continued primacy accorded to concepts such as "societies" and "cultures".

It is mainly among anthropologists who dare to involve themselves in *reflexive anthropology* that these issues really matter to the extent that conventional units of study and accompanying methods of analysis are subjected to radical and ongoing critical scrutiny. Crick proposes that "anthropology must be turned back on itself" (1982: 15) in an effort to become more conscious of the assumptions underlying anthropological practice. And Scholte (1974) argues for a "reflexive anthropology" which is willing to investigate the anthropology of anthropology.

The "innocent" attitude towards scientific practice arises from a "scientistic" notion of social science:

> If "scientism" also considers itself empirical and problem-oriented, it usually assumes that facts are facts, that objective methods simply select relevant data without further affecting them, and that these "units of

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> analysis" can be processed to yield lawful predictions and functional norms (Scholte, 1974: 436).

If anthropology excludes itself from scrutiny, without trying to understand the social and ideological context in which it operates, it needs to assume an *epistemological gap* between the researcher and the object of study. The preoccupation with field work methodology may reflect this distancing process. Field work itself is often regarded as the most distinctive characteristic of anthropology (Clammer, 1984: 64), a reflection of the view of anthropology as a discipline which focuses on others.

In this paper I intend to discuss the "unit of study" debate or lack of debate in relation to my own experience of *volkekunde* and of anthropology, and to my present understanding of the implications of this debate for ethnographic research in a settlement in Gazankulu in which I have been involved since 1986. This research is part of a departmental project started in 1984 which includes studies in two other settlements in another district of Gazankulu. My argument in this paper could be summarized in the following way: *Strong theoretical models and emphasis on rigid method have fostered anthropological schools of thought at the cost of sensitivity to the complexity of social reality of which anthropologists themselves form a part.*

In accordance with the distinction between the context of field work and the context of text-creation one could distinguish between two aspects of the unit of study: the *unit of observation* and the *unit of analysis*. This seems to correlate with the *micro/macro* distinction. These distinctions are useful because of the persisting tendency to generalize from a small ethnographic sample to a supposed larger aggregate which is often, ontologically speaking, located only in the mind of the anthropologist. The ethnographic and analytical context are, in other words, too often treated as being similar.

In a recent publication explicitly devoted to the issue of micro and macro levels of analyses (De Walt & Pelto, 1985), the editors try to accommodate conflicting points of departure by presenting them as complementary. The editors of the collection of essays propose a *systems approach*. The problem with this holistic view on the unit of study issue is that important theoretical and methodological questions are left unanswered. The relation between differing theoretical points of departure should rather be seen as one of competition, as expressed by Comaroff:

> The question of the "unit of study" far from being a methodological nicety, is a consequential theoretical matter. For fields of enquiry are never naturally given; they always reflect substantive assumptions about the constitution of the "real" world (1982: 144).

In the *South African context,* for example, conventional and institutionalized concepts regarding "race" and "ethnic groups" have influenced the work of both social anthropologists and *volkekundiges* (cf. Sharp, 1980: 2).

Listening to some of the voices in the unit of study debate may be useful. Let us first look at the approach in which I received most of my training and which is a prime example of what may be called methodological collectivism.

2. Theoretical approaches

2.1 Volkekunde

The point of departure of *volkekunde* is the notion of a *volk* (ethnos). A *volk* is considered to be a social unit bearing a specific cultural tradition with its own character and momentum. The *volkekundige* with the most influence in recent years, P.J. Coertze, describes the relation between *volk* and culture in his theoretical book:

> Through the medium of their cultures people ensure themselves of a happy biological, social, intellectual and spiritual existence in ethnic groups. The total culture of an ethnos is, therefore, the form which its life takes as an integrated whole through which it ensures itself continuing existence (1980: 155, my translation).

In the *volkekunde* paradigm knowledge of the *traditional culture* of an ethnos is necessary in order to understand its existence adequately. Contact with other similar units may lead to change and adaptation, but this has to be seen as a process of either reintegration into a new unit or disintegration and chaos. The aim of *volkekunde,* therefore, is to find examples of pure culture and to describe and explain them as ethnic phenomena. In the South African context the "tribes" of black ethnic groups are the main area of research of *volkekunde*. The problem of intra-tribe and inter-tribe variation is dealt with by means of the concept of the *kerngroep* (core group) which represents the pure life of the group. Some tribes are purer representatives of their *volk* than others and some tribesmen know their culture better than others - it is therefore important to choose informants correctly: those which are representative of the culture. Their testimony is used as the unit of observation which is then generalized to describe the culture of the *volk* as the unit of analysis.

In this way a circular argument takes shape: in order to present the pure life pattern of a *volk,* it becomes necessary to find examples of the purest parts of that *volk* and to underscore this finding with historical material. The unit of study is, therefore, not open to query, but is accepted as a given, primordial fact. Ethnography, in this sense, is the affirmation of ethnos theory. Variation, change and other phenomena not in line with the expected result are systematically accommodated as lesser issues and subordinated to the major issue - the existence of the ethnic unit (see e.g. Boonzaaier and Van Vuuren, 1985 on a research problem in Gazankulu).

Having been trained in *volkekunde* and having only recently come to see the defects of that paradigm, it was important for me to start practising "real" anthropology. For me, this meant participant observation in a settlement, not having to stay outside the research area and not being dependent on informants. Of course such motivation is not sufficient for a sound understanding of local conditions. The need to distance oneself from a school of thought on academic and political grounds is part of a personal development process which may use up valuable time and energy. Moreover, the reaction against a paradigm, in my case *volkekunde*, may unwittingly lead to the exclusion of certain lines of research. Possibly the rejection of the idea of happy ethnic units in their homelands has made me more critical of life conditions and nationalism there, which would not have been the case in other circumstances. Although I reject the way *volkekunde* understands culture and ethnicity as primordially constituted, I cannot claim to be completely sure of how to interpret the working of the tribal system and accompanying group consciousness.

2.2 Cultural and social anthropology

It would be a major fallacy to regard the *volkekunde* paradigm as a uniquely Afrikaans phenomenon. In anthropology social units are often treated as given facts with readily discernible boundaries and characteristics. This is also true of the major recent streams of thought e.g. American cultural anthropology and British social anthropology. Notions of *tribe, culture* and *society* abound in anthropological publications.

The concept of "culture" as shared ideas and customs has attained an abnormally high theoretical status. The distinction between the complex Western way of life which was left to other disciplines, and the non-Western one which was claimed as the field of study for anthropology, fostered a view of people as living in homogeneous cultures, functioning as demarcated social units and staying more or less static. Therefore the need to discern between units of observation and of analysis was not felt, because it was possible to assume that the observed part was typical of the analytical whole (Kaplan & Manners, 1972: 191-194).

It was conventional in anthropology to see isolated cultural or social patterns as separate problems for study. The shelves loaded with monographs represent mainly what Keesing (1981) calls a *mosaic stereotype*: an image of the world as consisting of separate, unique patterns which together make a wonderful whole of differentiation.

The difference between conventional anthropology and *volkekunde*, is, therefore, not that clear. Basically *volkekunde* can be viewed as a local manifestation of perspectives on human society which were and are widely held in anthropology in general. This also applies to other approaches to the subject which are not discussed here, e.g. the structuralism of Lévi-Strauss which reconstructs and interprets *mechanical models* of traditional societies.

Just as in the case of *volkekunde*, our reaction against conventional anthropology may result in a position which denies the existence of culture or social structure. Although culture is not the homogeneous entity as depicted by an earlier generation, collective forms of consciousness do exist and have to be explained. An important question in this regard is the way in which such forms are constituted, upheld and changed.

2.3 Interpretive anthropology

In reacting against "scientism" and deeply influenced by phenomenology, interpretive anthropology or symbolic anthropology has recently become an influential mode of anthropological thinking. It is a movement away from the theory of social facts as objects to the theory of social facts as constructions, or texts of meaning. The work of Geertz is one of the most well-known examples of this type.

In a recent publication Keesing points out the isolating tendency of interpretive explanation in which cultures are *reified* as collective phenomena. What needs to be done is to relate systems of meaning to their social situation (place text in context). Furthermore, culture should not be thought of as evenly distributed amongst a population (resembling the conventional anthropological view of culture). Rather, knowledge has to be seen as asymmetrically distributed in relations of power between persons and categories:

> Cultures as texts, the rites and myths and concepts of a people, conventional tropes and images are deeply ambiguous, ... allowing alternative readings. Anthropologists, with their predilections for the exotic and their predispositions toward, even vested interests in, depicting cultures as radically different from ours and from one another, are prone to choose readings that fit these expectations and interests. We see in tribal cultures as systems of meaning the unique, the exotic - see philosophies, world views radically different from our own (Keesing, 1987: 162).

A good example of interpretive research in South African anthropology is the word of Hammond-Tooke. In a publication on the cosmology of "the Kgaga" (bordering on Gazankulu) he warns that culture should not be reified, but then proceeds to present a *closed system of ideas*:

> In a society like that of South Africa, in particular, there is a tendency to reify the concept of culture and to link it inevitably to a particular social group, giving the impression that the unique configuration of ideas is ineluctably associated with a certain people (1981: xiii).

> In a traditional system, such as that described in this book, and abstracted from the time flow of social change, the explanatory theories of the worldview tend to be couched in mystical (and personalized) terms and to comprise a closed system of ideas (*op cit.*: xiv).

The notion of *culture* as a closed system of *a people* recalls the basic epistemology of conventional anthropology and of *volkekunde*. A language group or a political unit is projected as a *cultural group* from the micro-basis of selected informants. The units of observation are extrapolated to units of analysis, in isolation from other units and cut off from historical events.

2.4 Methodological individualism

In reacting against the positivist view of anthropology as the study of the structure of society, the *process approach/situational analysis/extended case method/transactional analysis* suggested using case material of events as the base for analysis, not just as "apt illustrations" (see Holy, 1984: 20).

This reaction, which gave attention to the variable character of life in small-scale settings, arose after World War II with the growing realization of the *fluidity in human relations*. One result of this new insight was the reconsideration of the concept of *tribe*. Leach's study on the fictional nature of Burmese tribal units and Barth's study on ethnicity in Pakistan brought the *situational character of group identification* to the forefront of anthropological attention (Bennett, 1985: 31). Another effect of the new view of humanity was the emphasis on *conflict, on lack of harmony and on individual manipulation* as normal phenomena. Through these developments the concept of the unit of study was redirected towards the micro-level of the individual. According to the basic epistemological assumptions of methodological individualism, the events in a collectivity can be reduced to the strategic calculations of self-interested actors (cf. De Walt & Pelto, 1985: 2-3, Knorr-Cetina & Cicourel, 1981).

The problem with methodological individualism is its overemphasis on individual action as if people operate in a vacuum and can have or change whatever they wish. With reference to the explanation of ethnic boundaries by Barth, Sharp (1980) argues that methodological individualism remains basically part of social functionalism: social organization is still the focus (seen now from the perspective of the players and not of the group). In South Africa the membership of ethnic categories is, for example, not voluntary and therefore the circumstances and power relations surrounding the functioning of specific instances of ethnicity need to be explained. The main problem with methodological individualism is its reduction of social phenomena to psychological processes as well as its use of ahistorical explanation. The context within which the individual makes his/her decisions is then not regarded as a problem for study, but taken as given. The emphasis on the importance of context is one of the major contributions made by Marxist analyses to the debate on the unit of study.

2.5 Neo-Marxism

Neo-Marxism questions the conventional assumptions and units of study used in the humanities and argues strongly for including *context and interaction* in the study of social relations. One problem with this alternative construction of social reality, however, is the *dominance of a theory* which emphasizes the macro-context at the expense of detailed ethnography. Marxism in anthropology, as elsewhere, is not to be conceived of as unitary. Probably the very general division between *structural Marxism, political economy* and *humanistic Marxism* (cf. Ortner, 1984) is of use in conceptualizing some of the important differences within the Marxist tradition.

The *structuralist* view of reality does not allow for the particulars of specific historical situations. The concrete unit of observation is overruled by the *conceptual unit of analysis* (understood as the theory of nineteenth century capitalism in Europe). In anthropology these ideas were expressed by the *French Marxists* who stayed fairly safely within the boundaries of conventional Marxist thought, although attempts were made to distinguish new *precapitalist modes of production*. These studies were, however, undertaken in terms of the conventional demarcation of tribal communities and therefore threw no new light on the unit of study issue:

> ... these writers appear to share the view that "societies" as conceived by traditional anthropology are relevant units for analysis and that the comparative method, as used by nineteenth century evolutionists, allows them to treat existing "primitive" societies as *pre*-capitalist (Kahn & Llobera, 1981: 294).

In *political economy* two main lines of approach can be discerned: the *dependency theory* of Frank and the *center: periphery theory* of Wallerstein. These writers do take up the issue of specific historical situations with regard to the penetration of capitalism into colonial situations. Nevertheless, the complexity of all recent history is reduced to a one-way relation of domination: that of international capitalism over everything else. Anthropologists and historians eagerly accepted these models and thereby replaced the generalizations within societies with *global generalizations*. In this way the oversimplification and collectivism of traditional anthropology was turned in the opposite direction: instead of viewing units of study as homogeneous isolates; they were now seen as *homogeneous results of global processes.*

More recently a reaction against the rigid application of Marxian concepts has arisen, leading to a more creative variation of Marxist thought which tries to overcome the problems created by structural Marxism:

> Marxism, ... strong on the societal (and even international) perspective, was virtually devoid of an adequate "micro" theory capable of tracing out the connections and levels which intervened between the individual and the overall society (Worsley, 1984: 53).

This new development can be described as a *dialectical* or *humanistic* alternative in which there is still continuity with the basic epistemology of Marx, but in which adaptations are made to make provision for local differentiation and non-material factors. Concepts such as *class* and *mode of production* are retained as heuristic instruments, not as dominating theory.

In his attempt to move away from global generalizing, Wolf (1982) gives prominence to local history, but in the place of the global concepts he creates new categories: *kin-ordered mode of production, tributary mode* and *capitalist mode.* In this way he makes room for non-material relations which may be dominant outside capitalist systems (a là Godelier), but combines widely different systems to form one mode. When Wolf writes about the interaction between capitalism and the periphery in South Africa, he uses a blanket category, *the Zulu*, which conceals considerable local variation and complexity. His contribution is to contextualize the older functionalist image of homogeneous tribes, but it does not penetrate into micro-processes.

Other examples of this dialectical approach are the work of John Comaroff (1982) in Botswana and Van Binsbergen (1981) in Zambia. Despite great sensitivity to internal processes and the dynamics of articulation with the external context, Comaroff presents the Baralong cultural and social order as a *system.* Van Binsbergen approaches the unit of study problem explicitly in relation to the ongoing debate. He discusses the issue in a study of the subjective experience of group boundaries (ethnicity) in Zambia, which he shows to be fluid and situationally determined. Reflecting on the unit of study he reaches the following conclusion:

> Instead of a clear-cut unit of study ...., what remains is an awareness of interesting questions and possible sources of inspiration: an interdisciplinary outlook; and the intention to analyze the dialectics of consciousness not only among the people selected for study, but also within the realm of scholarship and ultimately in one's private reactions as a researcher.

479

> It may not be much, but as the solution to a non-problem it may still help us along (1981: 81).

From Van Binsbergen's perspective, interrelations and processes are of more importance than the demarcation of units or aggregates. This view admits the existence of self-demarcation without taking it as a unit of analysis or arguing it away by overemphasis on theoretical issues.

The writers in the humanistic orientation (cf. Sharp, 1985 for a South African example) share rejection of the traditional demarcation of primordially constituted units of study. But they do differ in the extent to which the local micro-context receives analytical preference. The more the theoretical model dominates, the less emphasis is placed on analysis of relations within the unit of observation. In "dialectical anthropology" a tendency to generalize about aggregates of people, theoretically categorized together on the basis of economic relations, remains.

3. Implications for research

One thing which a review of the theory and practice of anthropology has shown is just how preoccupied anthropologists are with the status of their own discipline. The unit of study debate is part of this tendency to be sensitive to the demands of the discipline and the requirements of innovation, rather than to the problems of social situations. In this sense the unit of study is a non-problem, because the search for such a unit is pointless, given the fluidity and complexity of social situations. This is not to say that the debate is fruitless. It is one way in which the pitfalls of our occupation are made visible. The recurrent tendency of theory to dominate debate and research is shown vividly by the various attempts to delimit final units of study. Theoretical concepts easily misdirect our research. In this case concepts of *aggregation* were shown to be theory-bound and therefore seldom reliable as research tools. Even such "units" as *households* cannot be taken to be natural social entities (Spiegel 1986). In our research in Gazankulu, local units of people sharing a stand were used to investigate domestic relations, but it soon became evident that social and economic relations transcend these local groupings much more than was anticipated, for people are often members of several households.

The demands of theory and the social context of science help to create social categories which are then projected back onto social reality as if these logical categories represent social groups, whether they be *volk, tribe, culture, society, household or class*. Such categories are based mostly on current political concepts (cf. Sharp & Boonzaier, 1988 for South African examples). Related to this tendency towards theoretical involution is the dominance of macro-level concepts in much anthropological writing. From a small base of observation anthropologists happily extrapolate to supposed larger units (Clammer, 1984: 66-68). The effect of these tendencies is that the human side of anthropology is forced into the background.

If there is no definite unit of study, what remains is a view of social reality as fluid, complex and changing. The chunks we cut out of that reality for our analytical purposes are neither neat nor complete. How these units of observation and analysis are to be demarcated depends on the research situation which includes the research problem, the way this problem is conceptualized, and the practical limitations of time and money.

In this century anthropology has depended on field work in small settings, typically a settlement, in which participant observation was done. The recent understanding of the limitations of participant observation has led to the need to find ways of documenting and understanding the personal and unique as well as the context in which interaction takes place. For this purpose the *extended case method in combination with contextual analysis* seems to offer rich possibilities (Van Binsbergen & Geschiere, 1985: 258). The study of extended cases requires participant observation in order to be able to document the circumstances in which events take place. This needs to be done over a number of years in a long-term involvement, in order to discern the processes and interactions which are relevant to the situation (cf. Foster & Scudder, 1979). Teamwork and comparison can be used to gain richer results than can be obtained by individual researchers (cf. Salzman, 1986). *Contextual analysis* is important in order to relate local particularities to broader forces which may be political, economic or ideological.

In our research in Gazankulu the emphasis is on obtaining understanding of the micro-context. For this reason we use participant observation for six-month periods in small social settings. Relatively small settlements (about 50 households) were selected for research. Additional information is collected by research assistants. This takes the form of surveys and descriptions of daily events and gossip. The mass of detailed data which this method makes available is rather threatening in its volume. It does, however, remain only a selection of a very complex stream of events. Using a computerized database helps to manage the material, but it still leaves us with the task of interpreting social events as outsiders. Also, the settlements chosen for research have to be compared with each other and placed in context. Already it is evident that there are major differences between the two districts of Gazankulu where our research is taking place. The history of the inhabitants was tied to game farms in the one case, and to tropical fruit farming in the other. Furthermore the opportunities for employment differ to a great extent, a factor which influences per capita income and ultimately social relations. But even the two settlements in the same district differ substantially due to differing relations with surrounding game farms. The construction of a relevant context which may help to explain the particular form of relations and circumstances in a settlement is a major task which requires historical and other specialized sources of information. This task still lies before us.

Long-term involvement, participant observation focusing on extended cases and contextual analysis seem to be a way out of the dead-end of *units of study*. However, the basic problematic nature of being an anthropologist remains and there is no finality in sight regarding the theories and methods which should be used. This is particularly evident when we admit the distance between the anthropologist and the people s/he studies.

> This relationship [of anthropologist and his "objects"] is often based upon unequal power and resources, is temporary and exploitative for the observer, but may have permanent and unforeseen results for the observed; and is asymmetrical, in that the fieldworker reports and analyses, but the observed may never have access to his data or have the opportunity to react or criticize them (Clammer, 1984: 65).

In the final instance units of study have to do with people with personal characteristics and feelings. Examining people as study objects becomes more and more difficult after involvement in their lives. Why should anthropologists, with their predilection for theoretical fashion and institutionalized difference, be able to understand the complexity of social life better than anybody else? Is their research involvement in social settings sufficient to claim authority in defining and explaining those situations, or to act as social critics? Knowing that we are part of the social context can, however, help us to work towards the integration of micro and macro contexts. The problem which remains is how to integrate these levels either explicitly or more implicitly without resorting to some form of caricature.

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482

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483

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Paradigmatic shifts in geography, planning theory and archaeology

32

Diverging geography and the radical contribution: A review

W.S. Barnard

1. Introducing a framework

Geographers like to view their discipline as one of paradoxes. As a distinct field of study, geography's roots go back to the Alexandrine specialisation of knowledge in the third century before Christ, but it became institutionalised only about a hundred years ago. Geography's domain ranges from solid granite domes to abstract residential preferences, a variety so staggering that most of the facts of geographical analysis are normally handled by an array of systematic sciences, of course within constructural frameworks widely different from that of geography. For most of history geographers felt more at ease with the natural sciences, but during the last three decades its centre of gravity shifted towards the social sciences and humanities, and the discipline was trapped in the crossfire of the contending epistemological traditions of the 1980's - those webs of assumptions, values and goals within which science is practised (Snyman & Du Plessis, 1987).

This review evaluates one of the most recent of these traditions, viz. Marxism, and its contribution to the structure and methodology of Western geography. In doing so the author claims no special expertise in epistemology: he is a searcher, the exercise is a reconnaissance and geography a fascinating field to explore. The evaluation is conducted on internalist lines: geography provides the point of reference, the independent variable, while Marxism is regarded as a dependent variable, not on its own merits but in terms of what it can contribute to geography. The review commences with a brief statement on the goals of geography, after which a framework is established within which the Marxist contribution may be located and eventually assessed.

Geography, according to David Harvey (1984), registers, stores, analises and synthesises knowledge on the distribution and organisation of phenomena of the earth's surface, or in the jargon of the discipline, earth space. Two

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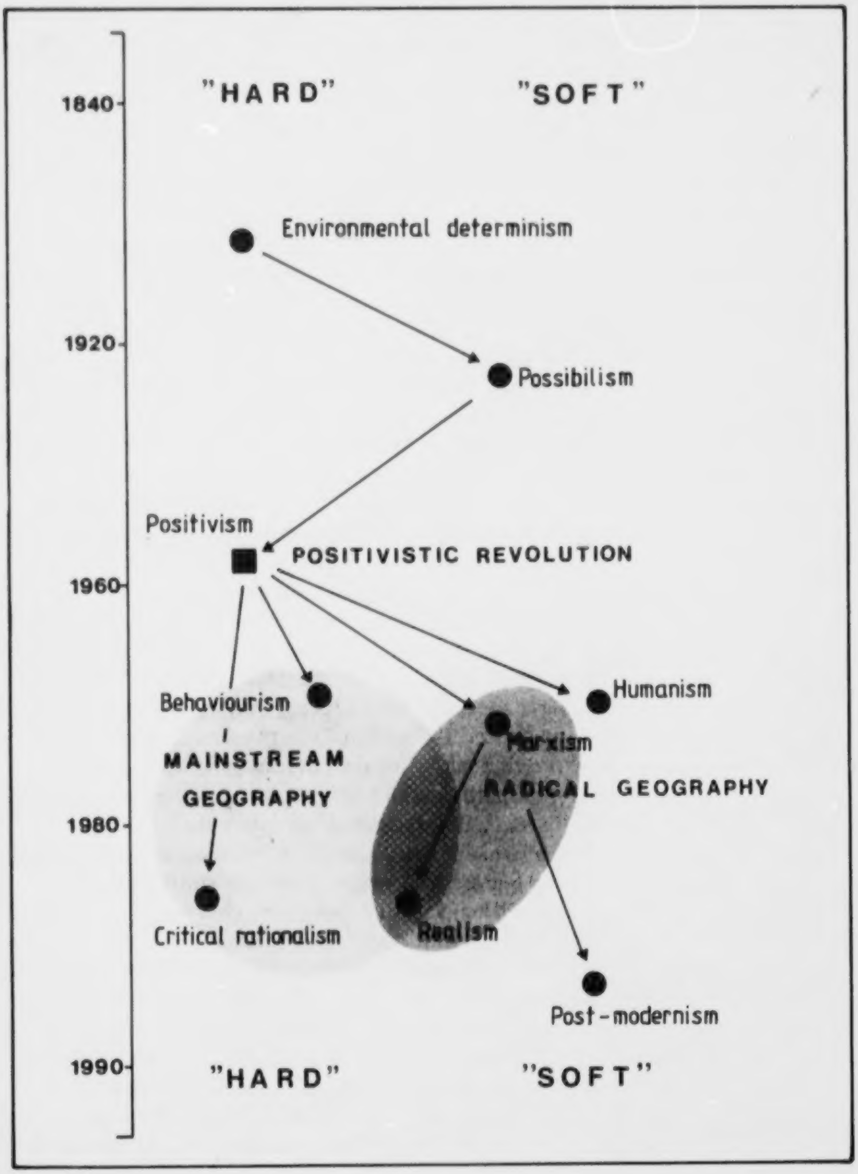
conventions guide the selection of "spatial" phenomena for geographical study. The first one dates back to Hellenistic times: the geographer's view of earth space in its totality, the tendency to interpret phenomena in terms of their spatial associations with other phenomena. The second convention entered the discipline during the 19th century: the tradition of man-relatedness, the view that significance in geography depends on what is relevant to man. "In geography", runs an often quoted adage from the 1920's by the German geographer Schmithenner, "man is the measure". Nevertheless geographical facts still need to be firmly rooted in earth space; therefore geographers prefer those aspects of man and his works which are solid and substantial.

Like all modern disciplines geography experienced shifts from time to time in the interpretation of its facts. A useful tool for teasing out the different styles of scientific behaviour which underpin these changes, is a model developed by the British geographer John Paterson (1979) and adapted here to accommodate changes which occurred since 1960. Paterson compares the course of geography during its last one hundred years with a stream which meanders from the one bluff line fronting on its flood plain to the other, loops between universalising approaches supported by vigorous methodology on the one side, and individualising approaches and methodological indifference on the other side. Paterson refers to the two contraposed epistemological positions as "hard" and "soft" systems. Notwithstanding the loops and turns, the stream maintains its downward sweep: the discipline grows in power and sophistication and less successful approaches are continuously eroded away and replaced by more successful ones. At the same time, other useful approaches may infiltrate beneath the surface and become seepages only to be intercepted and tapped by a new loop returning to their side of the flood plain. About 1960 the rhythm of meandering broke down as geography fanned out into a number of delta-like distributaries, new approaches not so much succeeding each other as simultaneously diverging into separate and different courses. Geographers using these courses, however, still look for links with older directions and their work is still interpretable in terms of "hard" and "soft" positions.

The stream model will be used to plot the trajectory of radical geography within the discipline, a scientific tradition which arose in the delta sector of the course but is difficult to understand without reference to the preceding meandering stretch and the delta apex. (See Figure 1).

2. The meandering stretch and the apex: Environmental determinism, possibilism and positivism

When the first geography departments started functioning at German universities in the 1870's, geography was an eclectic field of study which seldom


1840
1920
1960
1980
1990
"HARD"
"SOFT"
Environmental determinism
Possibilism
Positivism
POSITIVISTIC REVOLUTION
Behaviourism
MAINSTREAM GEOGRAPHY
Humanism
Marxism
RADICAL GEOGRAPHY
Critical rationalism
Realism
Post-modernism
"HARD"
"SOFT"

went beyond compiling inventories and descriptions of countries. Geographies, to be sure, were published, but their authors were historians, educationalists, journalists and aristocratic dilettantes. Great importance was attached to the less known parts of the earth's surface and from the 1820's onwards a rush of national geographic societies were founded in Western countries to promote discovery in far-away continents. Likewise their members were not geographers, but top civil servants, army officers, businessmen and the gentlemen of leisure who often undertook the journeys.

The founding professors, mostly geologists by training, were also the first geographers and had to transform geography into a discipline with its own structure, methodology and style of interpretation. They had to instill intellectual depth and discipline into what previously had simply been compendia. In an age when natural selection and evolution were excitingly new slogans, the early geographers succumbed to the temptation to regard the physical phenomena of the earth's surface as causes and its human phenomena as results. In 1905 a leading American geographer, William Morris Davis, went so far as to enshrine inorganic control and organic response in his definition of geography. In due course this approach became a scientific tradition which writers of later generations called environmental determinism, geography's rather naive version of the then fashionable social Darwinism.

The founding geographers also structured the various physical and human phenomena, which the discipline handled, systematically according to the class of phenomena and in the process created a range of physical and human subdisciplines which remained intact up till the 1980's. Because physical phenomena were regarded as the causal basis of geography, the environmental deterministic tradition legitimised the study of the physical features of the earth's surface in their own right and gave the physical geographical subdisciplines geomorphology, climatology and biogeography a content which differed little from the adjacent natural sciences, except for their emphasis on spatial aspects. They were also highly successful. By way of contrast the human subdisciplines - population geography, economic geography and political geography - were expected to explain human phenomena as results of environmental controls, which constrained their freedom of interpretation and kept them stunted and gnarled. In short, geography's initial model was the natural sciences, the discipline acquired a universalising habit and adopted a "hard" methodology of field mapping, instrumentation and inductive logic.

By 1910 geography turned away from environmental determinism. The generalisations of the determinists became untenably wide and poorly substantiated: they posed grand questions to receive trivial answers. The environmental determinism of German and Anglo-Saxon geographers subsequently gave way to the possibilism of the French geographers with their close links with the Annales school of history. The environment did not control, the French geographers argued, but only suggested possibilities, in other words the natural resources from which man chose according to his way of life. By centuries of interaction in a particular area, environment and man became symbiotically intertwined; the area acquired a regional personality struck like a medal on the image of its inhabitants. To understand a region's personality, the geographer should steep himself in its history, his approach was individualising and his methodology flexibly adapted to the context of the particular region. The swing towards possibilism, a "soft" system, constrained the physical subdisciplines, but its benefits accrued to regional geography rather than the rudderless human subdisciplines.

By 1950, after four decades of studying what was regarded as unique regions, geography found itself totally isolated from the world of science and its younger followers were grumbling in discontent. Regional geography, Kimble carped in 1951, was nothing more than "... trying to put boundaries that do not exist around areas that do not matter", an anachronism from the pre-industrial past (Kimble 1951: 159). Geography's problem, Schaefer wrote in 1953, was its claim of exceptionalism, the illusion that because it used both physical and human phenomena to identify unique regions, geography was different from other sciences. Geography was exactly the same as other sciences, although less developed because a mature science searched for laws. The return to a "hard" system surfaced first in geomorphology, the leading physical subdiscipline, which in the 1950 abandoned its sterile debates on the genesis of landforms for a new emphasis on measuring processes. When some young geographers transferred statistics to the human subdisciplines, they met resistance from the older establishment. The young geographers responded spiritedly by first defending quantification, then by advocating the use of normative concepts and eventually raising the construction of spatial theory to the pinnacle of the discipline. Their insistence grew into a self-conscious movement, which Burton in 1963, a year after the publication of Thomas Kuhn's *The structure of scientific revolutions,* called the quantitative revolution. It could more properly be called a positivistic revolution.

Geography espoused quantification at a time when it was highly fashionable in all social sciences, but the conversion was more dramatic and its impact more profound, the true point of delta divergence in Paterson's stream model. The initial quantifiers were American geographers during the late 1950's, while the synthesisers of the middle 1960's were Britons; European and practically all South African geographers joined the movement in the late 1960's. There were enthusiasm and growth in the number of students and staff at universities, new journals appeared, research output increased exponentially and firm links were established with the planning profession. But some developments had dire consequences for the following decades. Firstly the human subdisciplines replaced the physical subdisciplines at the cutting edge of research, while regional geography was virtually obliterated. Most geographers came to see themselves as mature social scientists with the same concerns as other social scientists and as sensitive to the contending epistemological traditions of social science. Secondly, the positivistic revolution enriched the domain of geography with constructs such as central place theory, spatial interaction theory and diffusion theory and broadened its methodology to include spatial prediction. Although such theory still referred to the "real" earth's surface, megaconcepts lost its oddness in a discipline steeped in empirical ways. Some geographers also acquired a taste for polemics.

3. The diverging delta: Between critical rationalism and phenomenology

Geography's positivistic revolution was so complete that a reaction was bound to happen. It was also so successful and spatial data lend them so well to quantification that many geographers were loathe to give up their newly acquired skills. Between continuity and change the discipline opened up to provide space for a spectrum of new epistemological traditions. Staying close to the line of "hard" bluffs, physical geographers maintained the empirical tradition, realised the necessity of a firm philosophical base for their work and in the early 1980's accepted Popper's critical rationalism. Still close to the "hard" edge, some human geographers' interest in problems of industrial location and migration theory lead them to decision making and eventually spatial behaviour. Most contemporary research is situated in the sector bounded by critical rationalism, modified positivism and behaviourism, the "hard" tending mainstream of the discipline.

The move away from positivism, the turn to "softer" systems, was a complex development. A small group of geographers went the whole way and ended up with the individual's subjective experience of earth space. By the middle 1980's they accepted a phenomenological philosophy and used place and landscape as major themes in work which came to be known as humanistic geography. A larger group of human geographers moved just beyond the halfway mark towards "soft" systems, were more strident in their critique of the "hard" sector and willing to experiment with methodologies and structures within the Marxist tradition. This so-called radical side-stream broadened in the 1970's, swept along some of the most productive geographers of the day, sprouted distributaries of their own and, by the middle of the 1980's, stabilised its following and position vis-a-vis other traditions. The rise of radical geography may be traced in three overlapping phases: in the late 1960's and early 1970's, a time when the bases of existing scientific traditions were probingly questioned, a time from the middle-1970's to early 1980's when Marxism was consciously

491

embraced and applied to construct an alternative theoretic base, and by the middle and late 1980's growing divergence within the movement, with at least some radical geographers renewing dialogue with the "hard" sector.

4. The phase of questioning: critical assault

Radical geography was part of a critical undercurrent which surged through social sciences during the late 1960's, the end of an era of confidence about the course of Western society. Against a background of decelerating economic growth, chronic unemployment and political conflict a new generation challenged the accepted diagnoses and cures prescribed by Western science. Many of the persistent societal strains were direct foci of the geographer's attention: regional backwardness, the urban crisis, the ghetto, Third World overpopulation, resource depletion and environmental despoilation. For young geographers it was no longer sufficient to approach these problems as research topics. They were perceived to be disfunctions of a system which was manifestly unjust and could only be changed by changing its underlying values. In 1969 some of them started a journal, *Antipode*, initially based at Clark University, Worcester, Massachusetts, and after 20 years of revolving editors, publishers and places of publication, still the flagship of the critical movement in geography. Just as important was a paper in 1970 by the American population geographer Wilbur Zelinsky in *Economic Geography*. Zelinsky warned that the growth syndrome of Western society was driving developing countries to disaster and called on geography, together with other sciences, to become involved in what he called "the Great Transition", a transformation of the values of Western civilization which for him was the only hope of survival (Zelinsky, 1970).

The questioning of Western institutions spilled over to Western science, an onslaught for which the New Left, neo-Marxism and critical social theory provided the ammunition. Geography, "this conservative, self protective little corner of science" (Peet, 1978: 15), was an obvious target because of its positivist revolution shortly before. The positivists still displayed the arrogance of power and tended to cow doubters into silence. Moreover, many thoughtful geographers found spatial theory not measuring up to either the old humanistic traditions of the discipline or to the new concern about values. The most trenchant critique came from the radical camp (Slater 1975) and may retrospectively be summarised under four points:

(1) Spatial theory was accused of reducing geographical phenomena to two dimensional patterns, a flat-earth geography which measured the outcome of events but failed to explain them.

(2) To formulate and apply spatial theory, positivist geographers were compelled to move away from the core of the discipline to borrow insights from adjacent social sciences such as economy, sociology and psychology. In the process geography was fragmented and its holistic traditions betrayed.

(3) The quantitative techniques used to verify spatial theory, made research elegance a goal on itself.

(4) The research model derived from the natural sciences was rejected as inappropriate to the social sciences. Its claim to value neutrality was rejected as misleading and dehumanising in a discipline which prided itself on the way it made man the measure of all things.

As alternatives to "objectivity", relevance and advocacy were acclaimed with ever increasing urgency. Delving in the past, radical geographers rediscovered the forgotten geographies of Elisee, Reclu and Pyotr Kropotkin, 19th century dissenters from the environmental deterministic orthodoxy of the time and advocates of the politics of anarchism. Journeys of exploration, another "neglected tradition", were undertaken to the ghettoes of Detroit and Toronto to share the problems of deprivation with the ethnic minorities involved, occasions of gripping tension and " ... state(s) of rationally controlled frenzy about the exploitation of human society" (Peet, 1978: 13).

Inevitably the questions were posed: Relevant to what and advocacy for whom? The conflicting responses triggered a debate which filled the columns of the British journal*Area* for a decade between 1971 and 1980. For mainstream geographers the answer was policy relevant and they accused radical geographers - a term first used by David Smith (1971) within context of the debate - of disavowing democratic values and preparing for a new and even more stultifying orthodoxy. For radical geographers, on the other hand, the answer was commitment to a truly liberated society and advocacy for people alienated by 20th century technocracies; in turn they accused mainstream geographers of perpetuating an unjust social order. Eventually radical geographers' quest for an intellectually thrusting alternative to the positivist tradition lead them to Marxism and ushered a new phase in the movement.

5. The phase of acceptance: Theory building

Explicit Marxism made its first appearance in papers published in the 1972 edition of *Antipode*, among which one by David Harvey on the persistence of the black ghetto proved to be pivotal (Harvey 1972). Harvey's conversion was the more remarkable because his *Explanation in Geography* three years previously, was already enshrined as geography's standard exposition of positivist methodology. Before long the trend was followed by an array of British and British-educated American geographers, among whom names such as Blaut, Bunge, Massey, Peet and Soja became prominent. The movement expanded, linked up with similar groups in France, Spain and Scandinavia, assimilated the involved vocabulary and constructs of historical materialism and broadened the thrust of their arguments over a widening spectrum of subdisciplines. Like Marxists in cognate disciplines, radical geographers accepted the materialist base, the centrality of the class struggle, the holistic perspective, the logic of dialectics and the emancipatory goal of the Marxist tradition, but most applied them in individual and usually overlapping ways. It is therefore difficult to discern clear groupings, the more so because some geographers changed their position over course of time. This review limits itself to brief statements on the work of the two radical geographers with the sharpest profile, viz. David Harvey, presently of Oxford University, and Richard Peet of Clark University in the United States.

The broad aim of radical geographers in the 1970's was to construct a new theoretical base for geography. "What it does need", Peet wrote in 1976 (1976: 164), "is a structure of philosophy (dialectical materialism), theory (historical materialism) and praxis (revolutionary action) capable of handling the enormous complexity of the spatial and environmental relations in which life goes on. Marxist geography begins to provide such a structure".

Of the two Harvey was the more influential at the level of philosophy and theory, where he did not so much reject spatial theory as to provide it with new explanatory content. In a 1972 monograph Harvey used Alonso's land rent curves of the property market to demonstrate that in open competition the poor would always be confined to lower quality residential areas and that residential segregation tended to become entrenched by the "red lining" policies of financial institutions and the zoning measures of local authorities. The underlying reason for the persistence of the ghetto, Harvey concluded, was the capitalist system and its incessant search for profit, which created an unequal distribution of wealth not only among people but also among regions. Spatial inequality, as much as social inequality, was seen to be inherent to the capitalist system for which it was necessary to reproduce regions of prosperity and poverty at both national and urban scale simply to create profit. In *Social Justice and the City* Harvey (1972) interpreted his empirical findings in terms of first a positivist and then a Marxist methodology, and concluded that the Marxist approach provided him with a more incisive explanation and a firmer connection between theory and fact. Later during the decade Harvey continued his quest for theory and came under the influence of Althusserian structuralism, although he later denied that he ever had been an Althusserian proper (Harvey, 1987).

While Harvey formulated theory, Peet excelled as advocate, the polemicist who exposed positivist epistemology as a "false consciousness" supporting a capitalist system racked with contradictions. Peet's style was assertive and his self-image that of a revolutionary but his revolutionary praxis tamely boiled down to practising a Marxist geography. Peet's position seemed to imply that the task of Marxist geographers was to prepare for the great transition by teaching, research and writing. Always the efforts of Marxist philosophy, theory and practice were targeted on the capitalist system, which was dissected, exposed and rejected, but the alternative society which was supposed to replace it, was barely mentioned. In a 1975 paper on poverty and inequality Peet spent five pages of critique on the capitalist system but only half a page on two alternative models, of which he found the anarchist model the more attractive and described it in two phrases (1975: 570) as the "... decentralised worker ownership of the means of production and a linked system of community control over environment". The subtly different emphases of Harvey and Peet, implicit rather than explicit, disclosed a rift between respectively the structuralist and anarchist versions of Marxism (Quani, 1982).

The debate between the proponents of the positivist and radical traditions continued during the 1970's and maintained its sharpness especially in British geography up till the 1980's. The convergent themes of unequal wealth and unequal power lead to the resurrection of political economy, while Wallerstein's construct of a world capitalist system with a spatial core, semiperiphery and periphery provided radical geographers with a link to political science and development studies. Mainstream geographers gradually acknowledged the perspectives of radical geography, acclaimed David Harvey as the most innovative geographer of the 1970's (Paterson, 1984) and created a new subdiscipline, social geography, to accommodate positivist and behaviourist geographers' commitment to societal welfare. For mainstream geographers, however, Marxism was not the only mode of explanation but one among several; their viewpoint opened the way to epistemological pluralism.

6. The phase of pluralism: Realism and beyond

By the middle 1980's Marxism was on the retreat all over the world and radical geography lost some of its vigour. In Britain the tradition retained most of its support but in the USA it dwindled to one of the smallest study groups of the Association of American Geographers. Grand designs and megatheory were no longer trusted and the Althusserian version of Marxism was badly knocked by Duncan and Ley's critique in a 1982 paper in the *Annals, Association of American Geographers*. The more tolerant intellectual climate of the times allowed radical geographers to take up different positions within the Marxist tradition and most opted to exchange theory building for the solid surface of the earth. Outside the Marxist tradition this "empirical turn" coincided with a renewal of both physical geography and cartography, the latter in the guise of geographical information systems. Mainstream geography remains committed to a welfare approach and is assured of its place on the ecological bandwagon. In this phase of diverging pluralism, four themes within or overlapping with radical geography, are notable, viz. those of realism, social theory, humanism and post-modernism.

There is no better illustration of geography's maturity as a social science than the way in which the discipline appropriated transcendental realism for its own ends. Although the basic constructs were formulated by Bhaskar, a philosopher interested in sociology, geography has its own realist methodologist in Andrew Sayer (1984) and a highly successful practitioner in Doreen Massey, a radical survivor of the 1970's.

Within a realist approach, according to Sayer, the way to explanation follow three ever deeper probing questions:

(1) What produces change?
(2) What makes things happen?
(3) What allows or forces change?

When a migrant from the Hercules suburb of Pretoria buys a house in Waterkloof, the transaction of buying produces the change, the buyer's aspiration to higher status makes the move happen and the policy of public authorities and financial institutions not only allow the move but direct it away from and towards particular areas. The banal example demonstrates that an important part of research lies in the identification of the agents, mechanisms and structures through which change come about and, even more important, an appreciation of their meaning. Regularities occur in the physical sciences, where the researcher addresses unchangeable mechanisms and structures, but they are impossible in the social sciences where human action continuously changes the relations as well as the conditions. Sayer distinguishes between abstract research (the unraveling of the necessary causal relations between structures and agents from the contingent relations) and concrete research (the explanation of specific events in terms of the interactions between structures and agents under contingent conditions). While abstract research exposes the causal powers and liabilities of social objects, concrete research provides causal explanations for particular events through the interaction of structure and agency under specific social conditions. Abstract research clearly has the greater explanatory power, but a synthesis with concrete research may, through case studies, contribute to an understanding of which relations are necessary and which are contingent. Sayer does not regard generalisation as particularly important and generalisations are in any case not verifiable; he also sees no intrinsic excellence in quantification although he does concede that there may be certain research areas where they are useful.

Realism provides a research strategy which superficially resembles causal structuralism; what it really is, is an open system within which voluntaristic decisions of man, whether taken individually or collectively, may lead to totally different outcomes, although a person may come to a particular decision under constraints of which he is not even aware. Most geographers who swung to realism, were "progressive" radicals who remained loyal to the Marxist tradition but felt curtailed by its deterministic undertones. They continued to work along the familiar themes of radical geography, but added one important initiative: a return to a study of regions or, in the jargon of the 1980's, localities. People or societies, goes the argument, are continuously changed by structures, mechanisms and agencies, and in the interaction between society and these variables, earth space also becomes modified or even transformed, albeit in areally varying and unequal ways. In due course earth space reciprocates by modifying society, also in areally unequal ways, and becomes an element involved in the processes of social change in its own right. *Geography matters!* is the title of a collection of readings by Massey and Allen (1984). In Massey's *Spatial divisions of labour,* perhaps geography's most influential book of the decade, she criticises the inability of both positivist spatial theory and structural Marxism to come to grips with the unique (1984: 6): "Both have an urge to normalise for a specificity of outcomes - and in geographical studies this can have major repercussions for a large part of what we are about is understanding unevenness, difference, place and locality".

The book focuses on the spatially variable ways in which British labour was changed since the Second World War by the restructuring of the industrial sector in specific localities under specific conditions of social stratification, work force composition, labour relationships and public policy. The emphasis is on agents (for example trade unions, employers' organisations, transnationals), mechanisms (policy, negotiation procedures, traditional labour relationships) and the underlying but all embracing struggle between labour and capital. Massey's approach is far removed from the inventories of industrial plants which characterised the regional geographies of the 1940's. No regions are demarcated and no distributions plotted on a map; the book of 330 pages contains only ten diagrams and its solitary map graces the cover. The theme of locality studies has been followed up in a major research effort, the Changing Urban and Regional Systems (CURS) project, which is administered from the University of Newcastle and directed at six selected localities in Britain (Cooke 1986). (Another example of an "empirical" study using a realist viewpoint, is discussed by Sarre, 1985 in *Area*).

The rise of realism did not totally supercede metatheory and some orthodox radicals criticised the "empirical turn" for its ghettoization of theory and abandonment of the Marxist banner (Smith, 1987). But the theoreticians also moved to a more tolerant position. In the first place, they readily concede the inadequacy of the original Marx for the complex world of the late 20th century, especially Marx's indifference to earth space and the environment. To rectify these omissions, Harvey (1984) called on radical geographers to develop a new historical-geographical materialism to replace the old, spaceless historical materialism. Others, such as Gregory and Urry (1985), took a parallel road but moved away from orthodox Marxism and interchanged subject and object: rather than injecting Marxism in the small world of geography, they want to integrate earth space into a wider critical social theory. In the second place the theoreticians also toned down their scepticism of empirical work within other methodological traditions in geography. Harvey (1984) suggested that mainstream and radical geographers could meet each other in an "objective materialism" with scientific integrity as a mutually respected value. A similar line was advocated in a 1982 paper in *Social dynamics* by the South African Jeff McCarthy.

Both the realist and structuralist variants respect formal theory and therefore have difficulty in relating to humanist geographers who, with their awareness of the lived experience and intentions of ordinary people, find themselves on the "soft" extreme of the discipline. Nevertheless some radical geographers did force a link during the 1970's with a theme in humanistic geography focusing on the literature of place, and a counterpoint was developed in South Africa by Wits geographers during the 1980's aimed at exploring novels, short stories and sketches set in the black townships of Johannesburg. They recorded vignettes of an alternative but vanished lifeworld (Hart & Pirie, 1984): the old Sophiatown which was "cleared" between 1955 and 1962 in a relocation programme but lived on in the memory of Modisane as "... proud indestructible soul, ... an undying symbol for those who loved Sophiatown, and those who feared it (pp. 46, 47)", and the shebeen (Hart & Rogerson, 1988), "... that twilight underground world of urban African life where all classes met ... (p. 19)". Although humanistic geography has been criticised as trivial and without context (Gregory, 1981), the Wits geographers left no doubt of their solidarity with the poor of the shanty towns of peripheral capitalist societies. (Also compare Western, 1981 *Outcast Cape Town*).

A small group of geographers moved beyond radical geography to a postmodernist position. With its roots in literature and architecture, postmodernism searches for a model for the political culture of the 21st century away from the 18th century Enlightenment, which provided the intellectual base for both liberal democracies and socialist authoritarian states. Its impact on geography is muted and the few papers supporting its cause (Dear, 1986;

Ley, 1987) review planning concepts and urban landscapes. For radical geographers, however, post-modernism is but a version of neo-conservatism (Graham, 1988).

7. Conclusion

Within geography radical geography has come to stay not only on account of its intrinsic insights but also because some people accept Marxism as an act of faith. Radical geographers' contribution to the output of the discipline since 1970 has been considerable and creative, although in First World countries this did not necessarily confer a higher academic status on the discipline. Supportive of a shift towards an interdisciplinary social theory, radical geography broadened the scope of geography, it blurred the dividing lines among the human subdisciplines and rejuvenated area studies and political geography. Radical geographers also challenged the monopolistic claims of positivist geography and nudged mainstream geography toward a welfare emphasis. But the radical advance stopped well short of their original goals. Although most geographers from other traditions are convinced of the value of multi-level explanation, they have problems with the selection of agents, mechanisms and structures and miss the internal scepticism, rigour and controllability of the "hard" sector of the delta model.

There seems to be two reasons for radical geography's lack of total success. The first one is related to radical geographers' ambivalent attitude to the future of geography. The "progressive" radical geographers who embraced realism, accept and support the reality of geography as a fully fledged social science within a larger constellation of sciences, sharing the epistemological pluralism of social sciences as a whole. In fact, they have come to occupy the position already attained by mainstream geographers in the late 1970's. By way of contrast, "orthodox" Marxists such as David Harvey, want to develop an apparently lower level "people's" geography parallel to his higher-level historical-geographical materialism, while the Canadian Elliot Hurst denies the existence of a separate discipline named geography and therefore rejects all speculation on the future of geography as waste of time. There is also a second constraint: the strong empirical tradition of the discipline and its still functioning connection with the natural sciences. Radical geography finds it difficult to accommodate man-land relationships and the resurgent physical geography of the late 1980's not at all. The present high ground of the discipline lies, just as before the positivistic revolution, in man-land relationships, but within an open, committed geography which dares to ask the great questions and wants to answer them affirmatively and humanely.

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33

Planning methodology: Empiriscism, rationalism and the practical imperative

John Muller

Introduction

The discipline of town and regional planning has, since the early 1970's claimed a proprietal right to a particular field of theory. This is the field of procedural planning theory which, inspired by Faludi (1973), has achieved popular recognition as the theory of planning - as opposed to theories in planning or theories for planning. Beyond its primary association with methodology, procedural theory has been widely perceived as distinctive in two senses. It is, in the first instance, classified as a theoretical domain independent of the substantive content of planning, and is, secondly, recognized as a genus of theory that has taken form around the concept of rationality. Procedural planning theory's right to theoretical status resides largely in its attachment to the philosophy and principles of rationality, and it is, interestingly, this attachment that has precipitated a paradigmatic impasse in the discipline. This impasse results mainly from the current radical critique of procedural theory which contends that the rational procedural model is severed from reality; that it separates itself from the socio-historical and political contexts of the real world and is consequently without functional utility.

The result of the critique has been a breakdown of the planning discipline's long-standing rational paradigm, and a somewhat fruitless search for a viable alternative. Town and regional planning is thus presently located within an interregnum; it is in the Kuhnian pre-paradigmatic stage awaiting the replacement of the previously dominant disciplinary approach with a new body of beliefs and theory. But, the rational model has been quite recalcitrant and is proving difficult to replace (Dalton, 1986). While various reasons as to why the discipline seems unable or reluctant to establish a new paradigm have been advanced (Alexander, 1984), the probable reason is the absence of the basic

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requirement for a shift in scientific belief - i.e. a conjunction of a new theoretical commitment and empirical change. If no new theoretical model is forthcoming - and there is presently little to engender expectations in this regard - the planning discipline could enter a period during which it floats rudderless into the future. This is a paradoxical and somewhat indefensible position for a discipline which has as its raison d'etre a consciousness of the future.

In addressing the impasse, the view presented in this paper is that the rational planning model need not be discarded. It is argued that the critique seeking to dislodge the rational paradigm is partially flawed since it places theoretical interpretations at the door of planning which are inaccurate. It is suggested that the rationalist rudder can be restored by adopting an essentially pragmatic and practical approach to the tenets of rationality. For purposes of this argument, the development of procedural planning theory - of the critical constituent methodologies and their correlation with rationalist and empiricist epistemologies - will be reviewed, and the essence of the critique of procedural theory will be presented.

Development of planning methodology

Early positivism

The first murmurings of methodology in modern town and regional planning practice are found in the survey-before-plan doctrine of the Scottish biologist, Patrick Geddes. Introduced around 1915, this uncomplicated model was accepted and adopted as the methodological convention in planning until the post-1950's era, and was evidently construed as a form of scientific method which gave "scientific validation" to the work of planners (Ravetz, 1986). The model is characterized by Taylor as a "crudely positivistic view of scientific method" (in Faludi, 1986: 38). This characterization is well founded since Geddes drew his approach form the diagnosis - before - treatment medical analogy and in addition, applied biological principles to the growth of cities; in which context Mouton (1989) has stated

> A central thesis of positivism has always been to compare man and society to an organism; alternatively, it is suggested that society is similar to the human body. In the analogy of society as an organism, which underlies the equation of the social and biological/medical sciences, we find the central metaphor of therapy: the aim of the social sciences is the healing of the social diseases/evils of society. And as in the case of medical practice, effective treatment is based on accurate diagnoses.

The positivistic element is, in this sense, evident in the curative connotations that attach to the civic survey component of the Geddesian model. An intellectual correlation between Auguste Comte and Geddes is drawn by some commentators: "Not only can we view Geddes' *Survey before plan* as a variant of Comte's *Savoir pour prevoir ainsi que pouvoir!*, Geddes also emulated the lattter's belief in definite historical tendencies which one should discover so as to plan accordingly" (Faludi, 1982: 85); "Experiential Holism was based on the Comtian-influenced empiricism of Patrick Geddes ..." (Weaver, Jessop, Das, 1985: 155). A third commentator provides a more pragmatic interpretation: "He (Geddes) looked for the practical implications of new knowledge and, in doing so, he opened up vistas of new possibilities to those engaged in concrete or practical disciplines ... Geddes ... wanted to be a "practical" social evolutionist ... seeking, not an intellectual haven, but the practical outcome of his sociological perspective" (Meller, 1981: 46, 49-50). While the latter view is arguably the most accurate representation of Geddes' personal philosophy, the attribution of positivist pretensions to Geddesian methodology is clearly not misplaced. The emphasis placed in the methodology on tangible and observable phenomena bears adequate witness to this.

The Geddes influence extended across the Atlantic to the Regional Planning Association of America. One of the RPAA leaders of the 1920's, Clarence Stein of Radburn fame, painstakingly developed a project procedure starting with "purpose" and concluding with "appraisal" of the implemented plan. His methodology was thus essentially empirical as indeed was that of Abercrombie in Great Britain. The latter is credited as having been the first to advocate a system approach to planning (Breheny and Batey, 1981: 113). As a practitioner, he emulated Geddes in emphasizing the practical importance of the survey function in planning, as did the academically-oriented Lewis Keeble some years later (Marshall and Masser, 1981). Back in the United States, the National Resources Planning Board published, in 1943, a guide for community planning under the title *Action for Cities*. The guide recommended a three-step methodology for planning: determination of goals, determination of needs and programs of meeting needs. Although the NRPB guide "had not completely captured the concept of planning as something more than survey and analysis" (Funigiello, 1983: 166) it contributed to the evolution of planning methodology by entering the domains of social activity, public opinion and economic processes. Its procedure was grounded in the practice of planning, and its positivistic bias, like all methodologies of that time, was clearly discernible.

Rationality

The analysis by Meyerson and Banfield of the work of the Chicago Housing Authority undoubtedly represents a watershed in planning thinking and methodology in the twentieth century. With the publication in 1955 of their *Politics, planning and the public interest* the concept of "good planning" as "rational decision-making," and the notion of rationality in planning methodology were introduced for the first time. By a "rational decision" say Meyerson and Banfield (1955: 314) "we mean one made in the following manner:

> 1. the decision-maker considers all of the alternatives (courses of action) open to him; i.e. he considers what courses of action are possible within the conditions of the situation and in the light of the ends he seeks to attain;
>
> 2. he identifies and evaluates all of the consequences which would follow from the adoption of each alternative; i.e., he predicts how the total situation would be changed by each course of action he might adopt; and
>
> 3. he selects that alternative the probable consequences of which would be preferable in terms of his most valued ends.

This process of rational decision making is embodied in Banfield's four-stage model of rational planning. The first stage is "analysis of the situation", the second "end reduction and elaboration", the third "design of courses of action", and the final stage "comparative evaluation of consequences". The sources from which the authors derive their model are Herbert Simon and Talcott Parsons.

Simon, who is credited as the first to articulate the problem of rationality in decision-making (Friedmann and Hudson, 1974), states unambiguously that the "task of decision involves three steps: [1] the listing of all the alternative strategies; [2] the determination of all the consequences that follow upon each of these strategies; [3] the comparative evaluation of these sets of consequences" (Simon, 1945, second edition, 1965: 67). The congruency of this 3-stage process with the three decision steps outlined by Banfield is clear. The Parsonian influence is reflected in the extract from Parsons that Banfield uses to illustrate the source of his conception of a rational decision: "action is rational in so far as it pursues ends possible within the conditions of the situation, and by means which, among those available to the actor, are intrinsically best adapted to the end for reasons understandable and verifiable by positive empirical science ..." (Parsons in Meyerson and Banfield, 1955: 314). There is thus an amalgam of rationalist recourse to reason and empiricist application of real world experience in this, the initial rational planning model.

Davidoff and Reiners' seminal work on a *Choice theory of planning* (1962) describes rationality in two senses: increasing the reasonableness of decisions and involving full knowledge of the system in question. They see the latter as approaching a model of full rationality (identification of all alternatives relating to all ends) but tend to favor acceptance of Simon's more limited "satisficing" or "bounded rationality" concept of rationality. Satisficing involves a quest not for the best of all alternatives, but for a course of action that is "good enough" or satisfactory. The choices that make up the Davidoff and Reiner planning process are exercised at three stages: the first is the selection of ends or goals; the second is the generation of alternatives and selection of the alternative best able to meet the goal; and the third is programming for the implementation of the goal. The authors make the point that "each of these choices requires the exercise of judgement; judgement permeates planning. We will show the need for and some means of rendering judgements explicitly and with reason" (Davidoff and Reiner, 1962: 12). This statement is remarkably apposite in relation to Brown's recent "judgement model" of rationality (1988) which proposes that the exercise of judgement provides an adequate basis for a model of rationality.

Cooke (1983) sees Davidoff and Reiner's choice process as typical of the confusion in planning theorizing which mixes normative theory with positivistic interpretations of the subject matter. He discerns philosophical inconsistencies in a theoretical process that advocates the application of rationalist principles, yet holds that positivistic empirical testing of the principles is needed to produce valid knowledge on which to base plans. The *Choice Theory*, is on the other hand, presented as the most articulate expression of the rational planning model by Alexander (1984: 62). Divergent opinions of this sort give credence to the opinion that the planning fraternity's use of the term rationalism is inconsistent and confused, and is different to the interpretation used by the social science disciplines (Reade, 1985). This matter will be addressed later in the paper.

Synopticism and systems

The rational-comprehensive or synoptic approach - a coupling of comprehensiveness with rationality - moved into the vanguard of planning methodology in the 1960's and 1970's. It became the quintessential model of procedural planning theory, was virtually synonymous with planning thought and sought to apply logical positivism to society. Rationality was defined on the basis of positive knowledge, which was claimed to be objective and universal (Weaver, Jessop, Das, 1985). The approach was further consolidated as the fields of operations research, cybernetics and systems analysis took hold in the planning discipline in the late 1960's. The introduction to planning of systems thinking (Emery, 1969), systems approach (McLoughlin, 1969), systemics (Catanese and Steiss, 1970) and the systems view (Chadwick, 1971) carried rationality and comprehensiveness in planning methodology to a zenith - while concurrently (and paradoxically perhaps) the radical critique started chipping away at the cornerstones of the model.

McLoughlin provides the clearest exposition of the application of systems and cybernetics in the planning process. While his method has been epitomized as one of rational choice (Marshall and Masser, 1981), it is interesting that the "philosophical problem of rationality" receives no more than passing reference in a single paragraph in his book. Chadwick, on the other hand, addresses the issue of rationality in decision processes fairly fully. He derives his rational model of systemic planning from a simplified systems analysis procedure, which is an extension of an applied scientific research process that is in turn a modification of the "hypothesis-observation-test hypothesis-modify" cycle of scientific enquiry (1971: 66-68). Chadwick thus accords his planning methodology the attributes of scientific method supported by a form of normative rationality theory. Catanese and Steiss see the systemic planning process as reflecting the concern of systems analysis with scientific method, and follow the systems input - conversion - output framework in their model.

Methodology in planning has thus developed as a mix in which the procedural requirements of scientific method, of empiricism, of positivism stands alongside various notions of rationalism. Camhis (1979: 17) states:

> the meaning of the term "rational" - in rational planning - can be related to philosophical rationalism, as well as to the "rationality" which springs from the development of science (empirical science).

Dalton (1986: 147) puts the view that

> rationality (in planning) in twentieth century Western society derives further meaning from logical positivism. In research, rationality depends upon fidelity to accepted procedural requirements of the scientific method in order to discover and apply general laws.

The rational-comprehensive procedure became the butt of attack on a number of grounds. The earliest offensive emanated from the incrementalists who, in deriding the procedure on the basis of man's restricted problem-solving capabilities, proposed the method of successive limited comparisons (Lindblom, 1959) as a superior methodology. This process of incremental decision-making has features in common with Popper's piecemeal engineering approach. Etzioni (1967) sought to combine the positive elements of both the rational-comprehensive and incremental models in his synthetic mixed scanning strategy. The most recent attack has been launched from the radical left and inasmuch as the present paradigmatic predicament in planning stems largely from this onslaught, the radical argument warrants some attention.

The radical critique

As stated in the introduction, the burden of the current critique of procedural theory is in the perceived inability of the procedural model to recognize and accommodate socio-political reality. This reaction is manifest most clearly in the writings of the planning theorists of neo-Marxist persuasion who propagate the replacement of the apolitical rational paradigm with an ideologically-founded approach.

The indictment of procedural theory is exemplified by the work of Scott and Roweis (1977). The functional tasks of planning are placed within a structural model of the planning process; a model which, in their judgement, is derived from general systems theory. Procedural theory is construed as engaging in a form of generalization, referred to as "indeterminate abstraction", in which assertions regarding goals, evaluation and the like are "plainly vacuous". Vacuous because the theory is, in Popperian language, structured for minimum rather than maximum refutability, and because it equates planning with abstract rationality that ignores socio-historical reality. Further, because procedural theory purports to transcend the socio-political relations of capitalism, it disguises the intrinsically political quality of planning intervention by transforming that intervention into no more than administrative decision-making rules. Substance and procedure are thus divorced; a separation that Roweis (1983: 143) describes as a "standard article of faith in mainstream planning theory". Rationality-rooted planning methodology accordingly fails to accommodate empirical reality.

In the eyes of Thomas (1979) the removal of planning from the "structural situation" of the real world reduces procedural theory to a distinctive form of thought and action (i.e. rationality based) that is unconnected with the substantive matter that makes planning necessary. Consequently, Thomas sees procedural theory as essentially "contentless".

Camhis, who constructs a comparative analysis around the idealism/materialism bifurcation, attempts to "indicate how both theory of planning and philosophy of science often serve in building up the ideological armour of capitalist society" (1979: 10). Idealism is presented as thought construction and a "kind of fiction", whereas materialism is equated with the "real existence of objects". Marxism is taken as representative of materialist theories, on which premise a theory of planning has - as a prerequisite - an understanding of reality. Rational idealist theories are perceived as abstract, independent of reality, and the rational-comprehensive model and procedural planning theory are accordingly censured.

The radical leftist critique is strong in an analytical/explanatory sense and is valuable in its insistence on a coupling of the substantive and procedural areas of planning. It offers however, little guidance to the planning discipline. The Fainsteins acknowledge that "Marxist theory and radical planning practice have lived uneasily with one another, in part because Marxism cannot provide a completely satisfactory guide for what planners should do ..." (Fainstein and Fainstein, 1979 in Paris 1982: 167). Certainly, the radical attack on rational procedural theory stops well short of any form of methodological prescription or alternative process. Camhis (1979) points out that Marx did not write an epistemological treatise and that the construction of a (or the) Marxist method presents difficulties.

Philosophical argument

Camhis' stricture of the rational planning model is predicated upon an interpretation of procedural theory as a state-structured obstacle to a preferred social order. It is this interpretation, this materialist critique, that is rejected on philosophic grounds by Taylor (1984). Scott and Roweis', Thomas' and Camhis' charges that procedural planning theory is (inter alia) vacuous, contentless and idealist, and that it serves to legitimize the capitalist State, is countered by Taylor in his distinction between conceptual (or analytical) theory and empirical (or synthetic) theory. He holds that the basic theory of procedural planning theory is a theory of rational method, which is arrived at not by empirical observation but "by conceptual or logical analysis of what would be required for a decision to be rational in a purely formal or logical sense. To put this another way, the basic theory of PPT is derived from an analysis of the concept of formal or logical rationality" (1984: 106). It follows therefore that procedural planning theory is essentially a conceptual theory. It advances a concept of rational decision-making/planning by presenting the distinctive and interrelated stages that must be followed in a logically coherent decision process. The basic theory of procedural planning theory is a purely formal (analytical) or a priori theory of planning; it is a definition of rational planning; it is not an empirical or synthetic theory of planning. Accordingly, procedural planning theory is not involved in satisfying the specific goal, or in setting forth the context of any particular problem. In essence, "... PPT is not a theory of substantive rationality and is therefore silent about what planning should, substantively, seek to do" (Taylor, 1984: 107).

This proposition has a certain dialectical appeal but, arguably, little of attraction to planning. Commenting on Taylor's thesis, Scott (1984) has difficulty in classing procedural planning theory (as presented by Taylor) as a theory of planning. Further, when the institutional and political contexts are removed from an analysis of rationality in planning as in Taylor's approach - the result is triviality. Huw Thomas sees in Taylor's perception of procedural planning theory, a parallel with analytical philosophy, which analyses, clarifies and refines but makes no claims about reality. "However, the approach to analytical philosophy must be one which is conscious of the political and moral context of philosophical debates ..." (Thomas, 1985: 59).

However intellectually arresting this philosophical discussion may be, it has not exercised a decisive impact on the radical argument. The basic contention of the radical critics is that the separation of procedure and substance in the rational planning process is simply untenable. They are of course right. "A planning procedure is a methodological response to an issue within prevailing societal conditions (substantive reality) and these conditions are transformed through planning intervention (procedurally structured) into new sets of conditions, which must then impact on subsequent procedural approaches" (Muller, 1987: 9). The joining of substance and procedure raises the intellectually associated issue of linkage between rationalism and empiricism, as well as the adaptation of the two epistemologies in the conventional or generally accepted methodology of the planning discipline. These issues require some scrutiny since they have in the end, a bearing on the validity of the radical critique and hence the practical viability of the rational planning model.

The classical traditions

Rationalism

The fountainhead of the rationalist tradition is found in the epistemological inquiries of Plato who, by strength of argument, concludes that true knowledge is attainable through the intellect alone. There is a "decisive step in Plato's reasoning that makes it appropriate to call him the father of rationalism: true knowledge, Plato insists, requires a move away from the sensible world to the world of "intelligibles". What is involved here is a fundamental contrast between the sensible world - the ordinary world revealed to us by the five senses - and a separate world of intelligibilia - a world whose objects must be apprehended by the intellect alone" (Cottingham, 1984: 17). Knowledge acquisition has thus to be not *a posteriori* (drawn from experience) but *a priori* (derived from independent abstract reasoning). The Platonic conception of a priori reasoning leads to the doctrine of innate knowledge. Despite his empiricist predilections, Aristotle does not provide an explicit rejection of the Platonic rationalist position. His approach to scientific reasoning seems to display an acceptance of a process of deduction from first principles, in which context Brown (1988: 44) records Aristotle's observation that "it is intuitive reason that grasps first principles".

René Descartes is, by general agreement, the most fertile source of rationalist philosophy. The celebrated Cartesian method of doubt which rejects the testimony of the senses, challenges judgment based on experiential evidence and accepts only the individual's knowledge of his own existence, leads naturally to a philosophy with rationalist underpinnings. Descartes' views (in his *Meditations on first philosophy* of 1641) on mind and matter, and on reason and the senses, offer "... the clearest statement in Descartes of the position that was later to be known as rationalism. Rationalism finds the key to knowledge, even of "sensible" things, in rational reflection rather than empirical observations" (Scruton, 1984: 37). The distrust of the senses is reflected in Descartes' oft-quoted example of a lump of wax that changes from a perceived solid to actual molten form, and that of towers which appear round while actually being square in shape. Cartesian epistemology accordingly gives primacy to the "natural light" of reason; to cerebral rationality. Spinoza and Liebniz also seek to describe the world exclusively in terms of pure reason.

Empiricism

The rationalist intellectual stance is of course rejected by proponents of the empiricist school who hold that all human knowledge is ultimately derived from sensory observation. Francis Bacon, the quintessential empiricist, starts his studies from the premise that all previous classical and medieval claims to knowledge are false and proceeds to the formulation of his inductive method of establishing universal laws on the basis of observed occurrences. His new method of scientific investigation, the *Novum Organum* of 1620, provides the platform for an analysis of sense-experience. His eschewal of intellectual rationalism is evident in the *Advancement of learning* in which he says of the classical philosophers: "their great error was, that they laid the blame upon the perception of the senses, and thereby pulled up the sciences by the very roots. Now the senses, though they often deceive us or fail us, may nevertheless, with diligent assistance, suffice for knowledge ... they ought rather to have charged the defect upon the mind" (in Quinton, 1980: 24-25). The Baconian inductive method of formulating general principles on accumulated observations of specific instances is acknowledged as the incipient model of the scientific method of today.

The emphasis on the use of empirical method and corresponding questioning of the exercise of reason is carried forward by Bacon's successors. In his influential *Essay concerning human understanding* (published in 1690), Locke is unequivocal in rejecting the proposition of *a priori* or innate knowledge. His description of the mind at birth as a "white paper void of all characters" (*tabula rasa*) is superbly illustrative of his stance. In arguing that the white paper can be inscribed only by experience, Locke clearly separates his empiricist epistemology from that of the rationalist school. Hume echoes Locke in insisting that the ideas of the mind are products of sensory input (called impressions) or are derivations of original impressions (memories). In Hume's 1748 *Enquiry concerning human understanding*, human reason is presented as the product of the relations of ideas which are simply tautologous propositions (twice ten is twenty), and of matters of fact which, being no more than substantive claims about the world, are open to denial.

At the time Kant entered the philosophical arena (18th century), science had established itself as an universal discipline; a discipline developed on the basis of findings established through the application of empirical methods. In questioning this tradition, Kant concludes that neither the precepts of empiricism nor of rationalism can produce a coherent theory of knowledge. In his judgement, the empiricist "who elevates experience over understanding, deprives himself of the concepts with which experience might be described ... "while the rationalist" who emphasizes understanding at the expense of experience, deprives himself of the very subject matter of knowledge" (Scruton, 1984: 141). Kant thus postulates the attainment of knowledge through a synthesis of intellect and experience: a meshing of a priori concepts with factual evidence.

Later philosophers such as Hegel, Schopenhauer, Kierkegaard and Nietzsche, Russell and Wittgenstein, Dewey and James, offer further contributions to the philosophic polemic, but the Kantian meeting of rationalist and empiricist epistemologies provides a logical point at which to introduce a brief discussion on the particular usage of rationality in planning.

Planning rationality

It was suggested previously in this paper that the earliest use of rationality in the planning process is found in the work of Meyerson and Banfield. There are in their rational decision-making process prescriptions that have some consonance with classical traditions. The decision process requiring consideration of all alternatives and all consequences (page 496) could, in conceptual terms, be construed as reflecting Cartesian method. Descartes' thinking is codified in four rules in the second part of the *Discourse on Method* and in the final rule he states that it was his practice "... in the process of searching and in reviewing when in difficulty, always to make enumerations so complete, and reviews so general, that I would be certain that nothing was omitted" (Descartes, translation Lafleur, 1960: 15). This can be equated with Meyerson and Banfields' requirement but, by the authors' own acknowledgment, the inspiration for their rational planning model is in the contemporaneous behavioural and sociological writings of Simon and Parsons.

Meyerson and Banfiled describe a plan as a course of action that can be put into effect and can thus be expected to lead to the attainment of the prescribed ends. Planning is described as designing a course of action to achieve ends, and "efficient" planning as a process that under a given set of conditions will maximize the accomplishment of the particular ends. "We will assume" say the authors "that a planned course of action which is selected rationally is most likely to maximize the attainment of the relevant ends and that therefore "rational" planning and "efficient" planning are the same" (Meyerson and Banfield, 1955: 314). The references to courses of action, given conditions and ends should be noted since they clearly reflect Parsonian thinking.

In the *Structure of social action* (1937, second printing 1961), Parsons embarks on a deep study of the theory of action in which he investigates, inter alia, the utilitarian mode of thinking - which places primacy on the conduct of the individual; on individualism. Individualism in society is extendible to individual acts, to discrete actions and ends, and to the means-ends relationship of the unitary act. The normative element in that relationship is identified by Parsons as rationality. There is "obviously a very solid common-sense foundation for attributing a large importance to rationality in action" says Parsons (1961: 57), "we are all engaged in multifarious practical activities where a great deal depends on the "right" selection of appropriate means to our ends, and where the selection, within the limits of knowledge current at the time and place, is based on sound empirical knowledge of the instrinsic relation of the employment of means to the realization of our ends." The essence of Parsons' thesis is that action is rational where it is pursued on the basis of scientifically sound knowledge of the circumstances surrounding the goal of the action.

The acceptance in planning circles of Parsonian social theory is particularly noteworthy since it embodies a number of distinctive features. The first is that the theory is not one of rationality but is a theory of rational action. It is thus not purely a form of philosophical rationalism resting on disengaged intellectual contemplation, but is a more practical rationality rooted in action-oriented principles. This makes the theory peculiarly pertinent to the social-engaged discipline of planning. The second feature supports the first inasmuch as it places rational action within a practical context - within "the conditions of the situation" - and in so doing integrates cerebral rationality with sensory-based empiricism. There is consequently in rational action theory a convergence of the two epistemologies, not the traditional divergence between the two. The situational or contextual element in this form of rationality is of obvious import to planning. The third feature is that pertaining to the utilization of "positive empirical science" for purposes of measuring the efficacy of alternative means of attaining the prescribed end. Deductive rationality thus enters the inductive world of scientific method. The exercise of reason is complemented by observation and experimentation.

These attributes of rational action theory, first grasped by Meyerson and Banfield in the 1950's, now constitute conventional and integral elements in the model of rationality used in planning. Harris (1985: 60) for example, states that rationality "is not altogether an abstract mental quality, since it is subordinated to the reality of actions and their consequences." Using Habermas' symbolic interaction model as a frame of reference, Forester (1985: 59) shows that an account of planners' behaviour can be given by "taking the problem of rationality to be a problem of action, more than one of epistemology and cognition".

Review

Having considered the concept of rationality within the context of classical philosophy and of planning methodology, an assessment of the veracity of the radical criticism of procedural theory and its rational model can be attempted. A significant component of the critique is that of the separation of procedural theory from the socio-political context of the real world. It follows implicitly from this argument that the rationality of planing must be of the classical genre; that it is essentially involved in introspective processes of reasoning, removed from real world influence. This is a questionable proposition. Classical rationality has as its purpose the search for truth, the discovery of true knowledge, which is at variance with the problem-solving objectives of planning methodology. Further, the Parsons-inspired rational action base of the procedural planning model is founded on "conditions of the situation" - on factual, concrete knowledge pertaining at the time in a specific context. The substantive element is thus inseparable from the procedural - just as the empirical is from the rational in the procedural model. Parson's reference to the use of "positive empirical science" in the verification of means (in relation to ends) is germane here.

Radical analysts advance the argument that procedural planning theory ignores the socio-political relations of capitalism by presenting planning methodology as a set of dispassionate administrative decision-making rules. This reference to rules has validity since "according to a classical model of rationality in the philosophy of science, a rational decision or belief must be based on an evaluation of relevant evidence by the application of appropriate rules" (Van Niekerk, 1989). Certainly, the methodology of planning prescribes a series of sequential steps which approximate rules, and these rules could be shown to preclude considerations of the political dimension - despite the ability of the rational model to integrate substance and procedure. The exclusion of the realities of politics in the practice of planning does indeed make procedural planning theory vulnerable to attack. The lack of political consciousness is perhaps also symptomatic of another problem: the absence of moral conviction, of an ethical commitment to a defensible system of values.

The notion that politics does not, and should not, enter planning activity has long been discredited but there remains an expedient adherence to the notion which is perhaps nowhere more obvious than in South Africa. Convention has long held the planner in this country the role of an apolitical technical expert, but reality rules otherwise. In contributing to the production of statutory plans - from governmental guide plans to local authority town planning schemes - the planner is, consciously or unconsciously, contributing to the implementation and maintenance of the government's objectives and can accordingly be perceived as having an ethical stance congruent with apartheid ideology. This supremacist ideology is a denial of the reformist principles on which planning has always stood (Muller, 1983), but has enjoyed covert planning support through the technical apparatus of rational methodology.

Conclusion

The planning discipline's procedural theory clearly has a number of defects. If the prognosis that these defects will result in the demise of the rational paradigm proves correct, the discipline will be left impotent for want of a methodology. Such a situation is unacceptable because town and regional planning is a practical discipline; a profession with the laudable aim of improving the quality of life of all people. In the final summation this aim is met, not by the introduction of theoretical constructs, but by the application of appropriate modus operandi. This is surely the fundamental prerequisite of any profession seeking to serve society. This is the practical imperative.

Accordingly, and in the light of the issues outlined in this paper, it is contended that the general preoccupation with rationalist philosophy and particular concentration on procedural planning theory should be superceded by a more practical concern with the methodology of planning. Methodology in practice - rather that procedure in theory - offers opportunities to respond to real world problems without becoming entangled in the trappings of theory or the requirements of rules. The practical methodology approach acknowledges the merit of the anti-procedural theory critique, but contrary to that critique, seeks not to discard the procedural paradigm but recast it in a form which sensibly accommodates the primary points of criticism. Because this practical approach need not attempt to be theoretically correct, scientifically consistent or rationally rigorous, it can be sensitive to context and can be structured to accommodate political and social value systems. The emphasis on practical planning procedure rather than procedural planning theory could thus provide a response to the plea made by Dalton (1986: 151) that "planners back off from the precise utilitarian definition of rationality and allow themselves to think of reason more philosophically as reflecting different ways of knowing or understanding the world (so that they can) then accept political or ideological doctrine, "everyday" or experiential knowledge, and humanitarian or empathetic understanding as valid as scientific knowledge ... allowing for such other form of reason then permits planning to deal with some of the dilemmas that the utilitarian rational paradigm does not handle well - such as political ideology and participation".

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35

Archaeology and the science of the concrete

Mary Hesse

The methodological debate

Like most of the human sciences, archaeology has passed in the last few decades through most of the phases of methodological debate that have characterized the natural sciences. There was a positivist and descriptive phase in the mid-20th century, which rejected earlier speculative and romantic tales of migrations, invasions and diffusions of culture having little conceptual or historical basis. Archaeology understood itself as what can be dug up and the immediate inferences therefrom. With the more sophisticated hypothetico-deductive method delineated in the mid-century for natural science (by Popper, Hempel, Nagel), the so-called "New Archaeology" of the 1960's and '70's' came to take a more relaxed view of the possibility of theories about the unobservable, so long as deductions from these could be empirically tested. In consequence, systematic, theory-led collections of test-data flourished. Statistical methods were ingeniously exploited to test hypotheses by means of apparently non-random distributions of objects, properties, and spatial and temporal arrangements. Such statistical studies often re-worked old data and derived illuminating new correlations.

The 1960's and '70's were, however, also the period in which philosophers of the natural sciences were coming to realize that empirical data are not simply given, but are theory-laden, that is, collected and interpreted in the light of some theoretical framework or other, and that if this is true in the natural sciences, it is true a fortiori in the human sciences. The process of observation-theory-prediction-test is circular, not linear: change of theoretical framework can lead to rejection of previously interpreted observations as well as observations leading to rejection of theory. Since archaeology is a human science, its data are always laden with interpretations involving human intentionality - flint flakes are related to tool-using, ditches to draining or defence, holes to foun-

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dations of buildings. Archaeology is also a historical science, and as in history it is misleading to expect rigorous lawlike correlations between types of human action and its social and natural environment. Inference typically takes place not to laws but to theories about the particular causes and antecedents of particular events, and takes place partly by analogy from similarities and differences between particular cases.

So far the methodological debate has followed the same lines as that common to all the human, or hermeneutic, sciences. But archaeology has the special feature that its theories must be about unobservable events that are inaccessible in time, and about social relations and mental beliefs that are inaccessible from mere material non-literate remains. Thus the methodology of inference from analogy with more accessible societies, whether documented or ethnographically describable, becomes crucial, and the problems of analogical inference become fundamental problems for all philosophy of archaeology. There is an added difficulty, shared by archaeology with the anthropology of extant societies, but to a greater degree. This is the problem of the dichotomy between so-called traditional or symbolic thought-forms, and the scientific rationalizing presuppositions of western thought, including of course the study of archaeology and anthropology themselves. To understand and reproduce the symbol-systems pervading non-western cultures that can be visited is a hard enough task; to discover it for societies distant in time is so daunting that positivist exclusion of any such attempt from a scientific archaeology might seem to follow necessarily.

The problem is this: how to infer from material remains and their natural environments to the socio-cultural features of the peoples who left them, a problem exacerbated by the fact that everything we know about prehistory and pre-literate peoples tells us that their conceptions of themselves and their societies were structurally very different from ours. How, in other words, do we infer from things to symbols and what they symbolize?

Symbolic and structural archaeology

It may be well to begin with some examples of studies which explicitly address this question. A model investigation of this kind[1] concerns a discovery in a Canadian Iroquois grave of the 12th century AD of two significant items: some bones of the Carolina parakeet, which had never been found alive north of the Great Lakes and is now extinct, and the stone bowl of a pipe. The context suggested the association was not fortuitous, and the author traces similar associations between birds and pipe-smoking through ever-widening circles of analogy and generalization, including 17th century literary and artifact evidence of pipes ornamented by bird skins and bird engravings, the Indian peace-pipe ritual, the smoking of entheogens [mind-altering plants], analogous modern Indian collections of significant artifacts, to the bird effigies and symbolisms associated with North American and Asian shamanism. Thus the original find of grave goods is put into a context of meanings linking time and space, within which it is interpreted as an indication of a form of symbolism in the 12th century, 500 years earlier than previous evidence had suggested for North America. Moreover, the investigation suggests that such bird/smoking symbolisms do not have to be spontaneously generated or diffused by cultural contact, but may reflect very ancient substrata of belief which persist in varying manifestations, depending on specific environments. The hypothesis is both suggested by and supports the data by interpretation and coherence. It is doubtless not the only hypothesis that could be suggested, but in this respect it is not different in principle from hypotheses in natural science, where theories are likewise underdetermined by data, and data are theory-laden.

Two other features of symbolism are suggested by analogical inference and can be illustrated by examples. The first is the hypothesis that symbolism is not a direct one-to-one relation between symbol and social meaning, but has a more complex transformational relation to the social structure. This may be illustrated by a study of Neolithic mortuary practice in England and south eastern Sweden.[2] The bones in barrows are not distributed as complete skeletons, but in non-random piles of different types: skulls, ribs, vertebrae, limbs, etc. Piles of different types are separated by definite boundaries marked by pots or skulls. The arrangement suggests an emphasis on the collective rather than the individual. On the other hand there are signs of differentiation between individuals, in that adult and child remains and male and female are sometimes separated, in apparent contradiction with the other type-classification. Cross-classifications of this kind are very familiar in social categorization, and may be taken to indicate some degree of contradiction between different manifestations of social order. In recent Marxist-inspired analyses the "contradictions" have been identified with those between social classes, elites, subject-peoples etc., but there is no need to restrict the idea to class-contradictions: kinship, trade, clan and other differences may provide equally illuminating hypotheses.

Tensions between cross-cutting differences may be represented in a symbol-system which serves to facilitate communication between groups, to reproduce existing social order and to legitimate it by attempting to mediate contradictions, and also to bring into consciousness latent conflict and hence to initiate social change. The authors of the Neolithic mortuary study follow Bourdieu in concluding that

> The natural and social world must appear to be self-evident. As we have seen, ritual activities by their very nature are one of the most effective means of carrying this out. Such activities serve to lay down a clear

> dividing line between the thinkable (the present social order) and the unthinkable (ome other social order)

The same example may serve to illustrate another problematic feature of the relation of the symbolic to the social. There is the difficulty for western thought of conceiving the connection between apparently arbitrary symbols and rituals and the social order of which they form part. As just pointed out, this is not a question of trying to "decode" symbols piecemeal into their "social meanings", but it does involve postulating some non-arbitrary relationships, however complex, between symbolic beliefs and actions and the (to us) more familiar world of family, production, economic exchange, and institutions of power. In the present example, correlations and ancillary evidence may suggest that piles of sorted and differentiated bones have an intrinsic relation to collective solidarity, but it is difficult for us to *think* the inwardness of such a relation. Similarly, pot-decoration (which is fairly accessible to archaeologists) has frequently been used to show how social boundaries and conflicts (which are not directly accessible) are represented, mediated or concealed. It is rarely the case that our dissociated aesthetic sensibilities can think complex connections between decoration and social structure; in other words, *verstehen* does not help us there as in many other areas of human intentionality. Hypotheses relating symbols to society have therefore to rest largely on correlation, analogy and circumstantial evidence across comparable societies.

As a form of scientific inference, analogy has traditionally been regarded as a frail and distant kinsman of inductive and deductive methods. There are, however, now signs of its rehabilitation within logic, philosophy of science and philosophical anthropology. In the next part of this paper I shall outline two of the developments that are particularly relevant to archaeological method. Firstly there is recognition of traditional thought as the so-called "science of the concrete", and secondly there are new justifications of analogical inference within scientific method in general.

The science of the concrete

Levi-Strauss begins his classic work *La Pensée Sauvage* by distinguishing between the logic and science of the *concrete* and that of the *abstract*. He makes it clear that in his view this is not a fundamental distinction between "traditional" and "advanced" societies, but is a discrimination that necessarily pervades all languages and cultures. All thought demands order, initially in the form of classifications:

> any classification is superior to chaos ... even a heterogeneous and arbitrary classification preserves the richness and diversity of the collec-

tion of facts it makes. The decision that everything must be taken account of facilitates the creation of a "memory bank".[4]

The science of the concrete classifies by means of immediate sensory qualities, by similarities and differences, by analogies of property and structure, and expresses abstract ideas by symbols drawn from the concrete. For example, early plant taxonomies emphasized obvious characteristics such as flower colour or medicinal qualities which gathered plants into groups, facilitated the recognition of herbal remedies, and provided a language of flowers as symbolic of the human passions. The science of the abstract, on the other hand, classifies by means of contiguities and causes rather than directly observable similarities and differences, that is it is metonymic rather than metaphoric, and it generates abstract theoretical structures from which a technical language and mathematical explanations may be developed. Plants are then classified by deeper characteristics not always directly observable, so that simple comprehensive taxonomies can be derived and related to objective features of plant evolution or inter-species fertility. The purpose of the classification is related to what can be done with nature (instrumental science), rather than what can be seen in nature or constructed out of it for the purposes of ordering social life. The language of flowers and all such metaphorical extravagances are discarded in the science of the abstract.

This, however, is to express the contrast much too sharply and from the point of view of "our" science, which positivist interpretation has regarded as superceding the science of the concrete because it is alleged to be the unique route to objectivity about what "really is" in nature. This propensity to relegate the science of the concrete to traditional thought in comparison with modern science can be challenged on two fronts. First, as Levi-Strauss himself emphasizes, all peoples of whom we have knowledge, from the paleolithic on, show evidence of using induction, hypothesis and testing on the basis of their classifications in the interests of exploitation of the environment. If logical thinking is constituted by the mind's ability to model the world, their capacity was often greater than ours. For example, questions about potential marriage-partners elicit immediate response, even though the highly complex kinship structures on which marriage rules are based often defeat the anthropologists' attempts to make them explicit. There is evidence from similar circumstances in modern non-western cultures that, when asked, appropriate rules can be produced, but only to satisfy the slow mental processes of the anthropologist. Non-literate societies, in fact, need to be able to manipulate as many complex mental models as we do.

Secondly, the concept of a symbol system needs to be re-examined. Traditional semiotics sees it as a kind of *code* in which signs, whether objects, words or actions, are taken to *stand for* something else which is their *meaning*. Such

5.3

a theory suggests that symbolic systems can simply be decoded piecemeal into their meanings, in much the same way as the discredited Fido-fido theory of word-meaning held that words stand atomistically for their referents. Even currently acceptable theories of language that depend on correspondence or satisfaction-meaning are bad models for symbolism, because they still imply a relation of "standing for" states of affairs that would make utterances true, although the units of language are now sentences and not words. These theories also introduce the additional element of "truth", which is problematic in the case of symbolism, because if symbolism is not reducible to "literal" concepts, it is not clear how judgments about its truth can arise.

A better model for the special features of symbolism is based on a more adequate non-correspondence theory of language. Such a model is implicit in many of the metaphors of recent archaeological writing, notably in Ian Hodder's *Reading the Past*. Hodder treats data as *text* to be read; all texts presuppose some *rules* of connection which it is the archaeologists' task to uncover; the interpretation of all texts depends on *context;* and most importantly the metaphor of *meaning* applies to both texts and objects:

> the meaning of an object is derived from the totality of its similarities and differences, associations and contrasts.[5]

I have argued elsewhere,[6] in relation to linguistic metaphor, that linguistic meanings should not be understood in terms of a relation of "standing for", but in terms of *meaning relations* such as synonymy and heteronymy, structural analogy and inversion, inclusion and contradiction. In this perspective words are like symbols (sometimes they *are* symbols), and ultimately the meaning relations in both types of system rest on perceived similarities and differences between objects, properties and contexts. Symbolism may lack syntactic and semantic rules of the kind studied in linguistics, nevertheless use of symbols is rule-governed, and since symbols and words have a similar semantic basis, symbolism may be said to be a primitive language. The crucial respect in which such a theory differs from standard linguistics is that it is *contextual*: in neither symbolism nor natural language does it make sense in general to ask of isolated objects, processes, actions, words, sentences, or even whole texts "*What* does it mean?", where "meaning" is taken to be something outside the context of use. Just as a poem's meaning is a function of its internal organization, which gains significance from a multiplicity of inexpressible resonances in the world and human experience, so it is with symbolism.

Analogical Inference

The meaning of symbols are therefore functions of relationships within a complex network which constitutes language, culture and social structure in general. Symbols communicate in particular, holistic contexts, and are typically learned by analogies and discriminations within particular situations, not by generalizable rules. The same emphasis on the local, the concrete, and the contextual emerges in studies of the place of analogy in modern science. These indicate that science too rests essentially on inference from the concrete, although it goes beyond it in ways that are connected with the overriding goal of scientific "objectivity"; unlike symbolism, the goal of science is successful prediction and control of the natural environment.

The logic of analogical inference in science is an extension of what W.E. Johnson[7] called *eductive* inference in contrast to *inductive* inference to generalizations and laws. The traditional problem of induction arose from the attempt to justify two types of elementary argument: the derivation of universal generalizations from particular data ("All crows are black"), and the derivation of properties of the next instance from observed instances of the same kind ("The next crows to be observed will be black"). The latter is eductive inference, and is weaker than induction to generalizations, requiring therefore weaker premises for its justification. It does, however, in common with all types of induction, involve recognition of instances *of the same kind*, and it is here that the notion of analogy enters at the root of all scientific inference. No two particulars in nature are alike in all respects; what traditional induction has assumed is that instances that are "sufficiently alike", or "alike in essential respects", constitute suitable sets of data for both inductive and eductive inference. Eductive inference from particulars to particulars makes the requirement of *analogy* between instances explicit, and can be generalized to give a probabilistic version of the standard form of analogical inference: if A and B are known to be similar in respects which outweigh their differences, then it is more probable than not that A and B will be alike in further respects. Further generalization of this logic gives the form of argument from models in science: if two physical systems (for example water waves and light) are similar in some properties and behaviour, then they may be assumed, at least for the sake of a useful hypothesis, to be similar in other related respects. Expression of the forms of these inferences begs many questions which remain to be discussed, but they may be usefully summarized for our purposes in a general *clustering postulate* which represents the underlying assumption of all scientific and indeed everyday inference: all other things being equal, assume that the world will continue to be more similar to than different from what has already been observed.

Eductive inference is not in conflict with other standard accounts of scientific method, but it is essentially local, particular and contextual, and makes fewer demands upon the universalizability of law-like relations and theories. It can therefore be transferred directly from the natural to the human sciences, which also require local, particular and contextual inferences. Judgments of sufficient analogy become crucial, as we have seen in archaeological examples. This should not be regarded, however, as a radical departure from scientific method, but as an extension of it which makes explicit what is always implicit in inductive and theoretical inference, however disguised in the elaboration of theoretical structure in the natural sciences. The method also retains an observational basis, unlike forms of constructivism which claim that all theories in the human sciences are merely in the eye of the beholder.

The pervasive character of analogical inference gives sufficient justification for the view recently expressed by archaeologists, that if the choice is to stop at the limits of data or to make speculative hypotheses on the basis of analogies with comparable systems, then it is better to make the hypotheses, even though there may in the end be no way of deciding conclusively which hypotheses are true. Neither is there any such way of deciding in the natural sciences. It should be noted in conclusion that although this prescription sounds like Popper's justification for "risky" hypotheses, it is not the same, because it appeals to an *inductive* type of reasoning generalized to include analogy, and this is a much better reflection of archaeological and anthropological practice than Popper's strictly hypothetico-deductive methodology. The science of the concrete postulates fundamental analogical structure between particulars in terms of which a logic of induction is possible, and this logic is common to the natural and the human sciences. The task that remains is to spell out from particular examples what are the criteria and constraints for good analogical induction.

Notes

1. A. von Gernet and P. Timmins, "Pipes and parakeets: Constructing meaning in an Early Iroquoian context", in *Archaeology as Long-term History,* ed. I. Hodder, Cambridge University Press, 1987, p. 31.

2. M. Shanks and C. Tilley, "Ideology, symbolic power and ritual communication: A reinterpretation of Neolothic mortuary practices", in *Symbolic and Structural Archaeology,* ed. I.Hodder, Cambridge University Press, 1982, p. 129.

3. *Ibid*, p. 151.

4. C. Levi-Strauss, *The Savage Mind*, London, Weidenfeld & Nicolson, 1966, p. 16.

5. I. Hodder, *Reading the Past*, Cambridge University Press, 1986, p. 138.

6. See my "The cognitive claims of metaphor, *Journal of Speculative Philosophy*, 2, 1988, 1; and "Theories, family resemblances and analogy", in *Analogical Reasoning*, ed. D.Helman, Dordrecht, Kluwer Academic Publishers, 1988, p. 317.

7. W.E. Johnson, *Logic*, Volume 3, Cambridge University Press, 1924, p. 43.

35

Theoretical progress in archaeology: When should the archaeological research community judge a theory or research programme to be better than a rival?

*Zak van Straaten**

Lakatos' model of the growth of scientific knowledge

The unit of change for Lakatos is not a paradigm (Kuhn), not a theory or hypothesis or basic statement (Popper), not theories and observation sentences (Quine & the justificationist fallibilists) but the Scientific Research Programme (SRP).

A SRP is a sequence of scientific theories such that each theory in the sequence is a modification of its predecessor.

A SRP has one central sub-component, two accompanying structures and a protective auxiliary belt.

The central sub-component is the hard core, which is a set of theoretical assertions/statements. Any theory which belongs to a SRP must subscribe to the truth of the statements in the hard core. The hard core is "conventionally accepted" and is provisionally not refutable.

The first companion structure is the negative heuristic which is a methodological principle which implies that the theoretical assertions in the hard core are not to be abandoned in the face of anomalies and counter-examples. You may abandon auxiliary observational hypotheses, or basic statements, or statements of initial conditions.

The second companion structure is the positive heuristic which is a partially articulated set of suggestions or hints on how to develop, expand or change the potentially refutable variants of the research programme, and a set of suggestions on how to make the refutable protective belt more sophisticated.

The current theory in a SRP always has a protective belt of auxiliary observational hypotheses, and is able to specify initial conditions. Testable

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predictions are derived from initial conditions and auxiliary observational hypotheses. The theory's auxiliary belt (TAB) is the set of all observational hypotheses which are testable.

A SRP is said to be progressive if its theoretical growth anticipates its empirical growth. Progressive SRP's have increasing empirical content and predict new facts. A SRP is said to be degenerating if its theoretical growth has slowed down and lags behind its empirical growth, and when it gives post hoc explanations of facts predicted by or discovered by a competitor SRP.

When should we regard a theory as falsified? Lakatos says we should regard theory T as falsified if:

> 1) another theory T' has excess empirical content of T; that is it predicts novel facts; i.e. facts improbable in the light of, or even forbidden by T.
> 2) T' explains the previous success of T, i.e., all the unrefuted content of T is included (within the limits of observational error) in the content of T'; AND
> 3) some of the excess content of T' is corroborated.

Lakatos believes we should not model the growth of scientific knowledge as a two cornered fight between theory and experiment. We should view it as a three cornered fight between two rival theories and an experiment. In the fight the world acts as referee. Lakatos wants to defend a realist view of science.

First application

Lakatos' model of the growth of scientific knowledge applied to Newton's research programme.

Lakatos has applied his model to Newton's global programs. I think he is very illuminating in his application and shall therefore quote him at length. All the quotations below are from Lakatos 1970.

> The classical example of a successful research programme is Newton's gravitational theory: possibly the most successful research programme ever. When it was first produced, it was submerged in an ocean ofanomalies" (or, if you wish, "counterexamples"), and opposed by the observational theories supporting these anomalies. But Newtonians turned, with brilliant tenacity and ingenuity, one counter-instance after another into corroborating instances, primarily by overthrowing the original observational theories in the light of which this "contrary evidence" was established. In the process they themselves produced new counter-examples which they again resolved. They turned each new difficulty into a new victory of their programme.

In Newton's programme the negative heuristic bids us to divert the modus tollens from Newton's three laws of dynamics and his law of gravitation. This "core" is "irrefutable" by the methodological decision of its protagonists: anomalies must lead to changes only in the "protective" belt of auxiliary, "observational" hypothesis and initial conditions.

I have given a contrived micro-example of a progressive Newtonian problem shift. If we analyze it, it turns out that each successive link in this exercise predicts some new fact; each step represents an increase is empirical content: the example constitutes a consistently progressive theoretical shift. Also, each prediction is in the end verified; although on three subsequent occasions they may have seemed momentarily to be "refuted'. While "theoretical progress" (in the sense here described) may be verified immediately, "empirical progress" cannot, and in a research programme we may be frustrated by a long series of "refutations" before ingenious and lucky content-increasing auxiliary hypotheses turn a chain of defeats - with hindsight - into a resounding success story, either by revising some false "facts" or by adding novel auxiliary hypotheses. We may then say that we must require that each step of a research programme be consistently content-increasing: that each step constitute a consistently progressive theoretical problem shift. All we need in addition to this is that at least every now and then the increase in content should be seen to be retrospectively corroborated: the programme as a whole should also display an intermittently progressive empirical shift" (Lakatos 1970: 133-134).

Lakatos' contrived micro-example

For even if experiments could prove experimental reports, their disproving power would still be miserably restricted: exactly the most admired scientific theories simply fail to forbid any observable state of affairs.

To support this last contention, I shall first tell a characteristic story and then propose a general argument.

The story is about an imaginary case of planetary misbehaviour. A physicist of the pre-Einsteinian era takes Newton's mechanics and his law of gravitation, (N), the accepted initial conditions, I, and calculates, with their help, the path of a newly discovered small planet, p. But the planet deviates from the calculated path. Does our Newtonian physicist consider that the deviation was forbidden by Newton's theory and therefore that, once established, it refutes the theory N? No. He suggests that there must be a hitherto unknown planet p' which perturbs the path of p. He calculates the mass, orbit, etc., of this hypothetical planet and then asks an experimental astronomer to test his hypothesis. The planet p' is so small that even the biggest available telescopes cannot possibly observe it: the experimental astronomer applies for a research grant to build yet a bigger one. In three years' time the new telescope is ready. Were the unknown planet p' to be discovered, it would be hailed as a new victory of Newtonian science. But it is not. Does our scientist abandon Newton's theory and his idea of the perturbing planet? No. He suggests that a cloud of cosmic dust hides the planet from us. He calculates the location and properties of this cloud and asks for a research grant to send up a satellite to test his calculations. Were the satellite's instruments (possibly new ones, based on a little-tested theory) to record the existence of the conjectural cloud, the result would be hailed as an outstanding victory for Newtonian science. But the cloud is not found. Does our scientist abandon Newton's theory, together with the idea of the perturbing planet and the idea of the cloud which hides it? No. He suggests that there is some magnetic field in that region of the universe which disturbed the instruments of the satellite. A new satellite is sent up. Were the magnetic field to be found, Newtonians would celebrate a sensational victory. But it is not. Is this regarded as a refutation of Newtonian science? No. Either yet another ingenious auxiliary hypothesisis proposed or ... the whole story is buried in the dusty volumes of periodicals and the story never mentioned again (Lakatos 1970: 134).

Positive heuristic

One may formulate the "positive heuristic" of a research programme as a "metaphysical" principle. For instance one may formulate Newton's program like this: "the planets are essentially gravitating spinning-tops of roughly spherical shape". This idea was never rigidly maintained: the planets are not just gravitational, they have also, for example, electromagnetic characteristics which may influence their motion. Positive heuristic is thus in general more flexible than negative heuristic. Moreover, it occasionally happens that when a research programme gets into a degenerating phase, a little revolution or a creative shift in its positive heuristic may push it forward again. It is better therefore to separate the "hard core" from the more flexible metaphysical principles expressing the positive heuristic" (Lakatos 1970: 136).[1]

The problem of methodology in archaeology

If Archaeology were an established science we would be in possession of a complete, consistent, and unified fundamental theory of all archaeological interactions.[2] The fundamental archaeological theory could be used to describe, not only actual observations, but future possible observations. It is a commonplace that in archaeology we do not have such a completed theory, and that we do not even have part of such a theory. Instead we have a number of different strategies for arriving at trustworthy explanations. Two of these are:

First methodological strategy

The first strategy consists in he description of social facts and the drawing of inferences from our knowledge of the facts. There is usually no basic theory which licenses the inferences. There is instead a collection of ideas which forms a system of background knowledge. It is here that support is sought for drawing inferences about social life. A basic assumption is that archaeological data are meaningful expressions of human culture and purpose. It is assumed that with sufficiently ingenious interpretations of the data, and with the assumption of an adequate sociological or archaeological interpretation of what we mean by "human', we can apply this concept to the domain of human in both current and historic times. One basic problem with this model is that it cannot as it stands generate scientific explanations; another problem that it is mentalistic. It is mentalistic in the sense in which Parsonian sociology is mentalistic, i.e. it relies on the language of common sense or folk psychology to understand "purpose", "intention", "wishes", "wants", "goals" of social groups. And disciplines which rely essentially on what is mental cannot be turned into sciences, (i.e. into disciplines which base their explanations on lawlike universal generalizations, and can specify in advance what would falsify a hypothesis or theory) while retaining the mentalistic, non-lawlike component.[13]

Second methodological strategy

Archaeologists study material phenomena. The relevant material things are in various configurations and forms. They are the matter and energy systems which are required for environmental adaptation. It is not necessary to presume one uniform concept of "human nature" for linking the present to the historic to analyze and assess social, biological, psychological, and material adaptation systems. The mentalistic non-lawlike component of the first strategy is, in this model of explanation, apparently illuminable.

Colin Renfrew points out in his introduction to Transformations: Mathematical Approaches to Culture Change, the point of this kind of approach is to attempt to move from "anecdote to analysis", from pictures and descriptions to explanation, and the analysis of material systems and behaviour. The point is to try to move away from a merely descriptive and eclectic methodology.[7]

Conjecture and refutation in archaeology

I think that those archaeologists who claim that there is no properly scientific methodology which is appropriate to archaeology as a whole are wrong. There is both a method and a methodology which is effective and suited to archaeology. It is the Popper\Lakatos method of conjecture and refutation which I have outlined above. It is a method which can apply effectively in those fields of archaeology where truth values and truth conditions are fundamental. And it is a method which can unite the two sets of methodological stances sketched out above.

For compactness let us call the method outlined above AM (for archaeological methodology). Let us suppose that the hypothesis:

(a) that AM is the only correct method in archaeology for generating the non-false, well corroborated, high- verisimilitude archaeological theories/hypotheses/basic statements is itself well corroborated.

What should we expect?

Some of the implications of the non-falsity of (a) would be that:

(i) The hypothetico-deductivism of AM would characterize the context of justification of an archaeological theory/hypothesis/basic statement. The strategies of AM and the implications which we may derive from it about how to proceed in a problem situation would also apply to the context of discovery.

(ii) We should expect in either the context of justification or of discovery of a theory or basic statement, that a great deal of time is devoted to attempted refutations and falsifications. Seminars and international conferences would (and should) be characterized by numerous and repeated attempts at falsifications or refutations.

(iii) We would not expect to find archaeologists having recourse to hundreds of cases of induction so as to be able to form one or more a generalizations based upon them.

(iv) The history of archaeology would, like most history of science, mainly contain accounts of refuted theories.

I have assumed above, in accordance with the basic presupposition of the methodology, that archaeological theories/hypotheses/basic statements are falsified or refuted in a variety of different ways.

The most obvious is by counter-example. Examples are either drawn from present day actual world cases, or from the archaeological record. I submit that this method occurs frequently in archaeological seminars, and at conferences.

Another type of refutation is to show that the proposed theory or hypothesis is incompatible with what is accepted as unproblematic current archaeological or scientific knowledge.

A theory or hypothesis could be incompatible with general logical principles, or general archaeological/economic/anthropological principles, which we have no reason to suspect as problematic.

In order to establish AM as the only correct methodology for archaeology a great deal more would be required than I have been able to demonstrate above.

Second application

Lakatos' model of the growth of scientific knowledge applied to the conflict between the research programme of the physical anthropologists, comparative anatomists, and paleontologists on the one hand and the research programme of the bio-scientists and geneticists on the other.

I take the following example from *The invisible ape* by Jerold Lowenstein and Adrienne Zihlman in New Scientist, 1988,[15] and in some places I have, in order to specify their example accurately, borrowed some of the terminology used by Lowenstein and Zihlman. The example fits the Lakatos mould, and shows how accepted evidence, which is apparently "absolutely obvious", for a theory, can be overturned by non-obvious, counter-intuitive evidence, which rests on a new technique. It particularly highlights the idea of a degenerating problemshift, as opposed to a progressive problem shift.

The original conjecture was that:

Chimpanzees and gorillas are more closely related to each other than to humans. This was held to be an obvious fact by the physical anthropologists because of the evidence:

They look more like each other. They are hairy; Men are smooth. Chimpanzees and gorillas walk on all fours;we walk on our hind limbs. Chimpanzees and gorillas have short legs and long arms but with very flexible hands; we have long legs and short arms with not very flexible hands. They walk on their knuckles; we do not, and cannot easily walk on our knuckles, since our hands cannot bear much weight. Their brains are small; ours are large. Their canine teeth are large; ours small. Their molars have thin enamel; ours have thick enamel.

For more than 100 years these differences convinced comparative anatomists and physical anthropologists of the truth of the conjecture. If it were true then our common ancestor the invisible conjectured ape would remain a mystery.

Was it a knuckle walker like Chimps? Or a biped like us? Or something very different looking?

According to Lowenstein and Zihlman the attitudes adopted by the physical anthropologists during the debate correspond to the four stages of human reaction to a bereavement: "first denial, then rage, next grief, and finally sadness and reluctant acceptance".[17]

Denial Stage: In the decade of the 1960's, Morris Goodman and Vincent Sarich (U.C Berkeley) showed by immunological tests of blood proteins that humans, chimps and gorillas are closely related. This implied that the three lineages separated only 5 million years ago.

The physical anthropologists denied this. They already had a 14 million year old human ancestor "Ramapithecus". This implied a divergence of apes and humans between 15-20 million years ago.

The molecular data contradicted the morphological evidence and fossil record.

Rage: The issue during the decade of the 1970's was: What kind of evidence best establishes genetic relationships through time; morphology and the fossil record or molecules?

Mary Clair-King and Allen Wilson (U.C. Berkeley) asked: How could humans look so different from chimps and gorillas when all showed the same degree of difference at the molecular level? (from 1%to 5%).

Answer: Only 1% to 5% of the DNA Genome is expressed as proteins. Between 95 and 99% consists of Introns or pseudogenes, so called "junk" genes. This is DNA that goes along for the ride, without doing any work. The "junk" genes replicate from generation to generation without affecting morphology. All this extra DNA is not much use to the organism but it is useful to researchers.

This DNA is not impeded by natural selection. It accumulates mutations at an even faster rate than the coding sequences of DNA. So it provides a fast biological clock for timing evolutionary differences, and a possible way of resolving the problem.

Three techniques called DNA sequencing, Mitochondrial DNA sequencing, and DNA hybridization,suggest three different lines of evidence which support the chimp/human similarity, and chimp/human versus gorilla difference.

DNA Hybridization bears same relationship to DNA sequencing as protein immunology does to protein sequencing.

It compares two large molecules by measuring their overall similarities and differences.

The double strands in the human DNA helix can be "melted" by heat into single strands. Then these can be "melted" with strands from chimpanzee molecules into a single strand. The hybrid is a chimp-human strand. The hybrids separate at a lower temperature (like sections of a defective zipper) than the human or chimp originals because of mismatches, in the base pairs. A 1' difference of tempt. represents a 1% difference in the sequence.

Charles Sibley and Jon Ahlquist (Yale) found that chimp-human hybrids are 20% more stable than chimp-gorilla or human-gorilla hybrids. This implies that gorillas split off from chimps/humans approximately 1 million years before the chimps/humans split from each other.

Sadness and reluctant acceptance: In the mid 1980's after 20 years of defence the physical anthropologists and paleontologists abandoned their original hypothesis that Ramapithecus was a human ancestor, and that chimps and gorillas are more closely related to each other than to humans. On the basis of the bone and teeth fragments that they thought supported their original theory, and which, with benefit of hindsight, led them astray, the paleontologists now proclaim that Ramapithecus is an ancestor to orang-utans.

Third application

Lakatos' model of the growth of scientific knowledge applied to the tension between the research programs of the archaeologists on the one hand and the research programme of the sociobiologists on the other.

My assumption is that human sociobiology, resting on a bio-science base of microbiological, genetic and biochemical theory, can be directly applied to humans now. I have argued elsewhere that the theory can be used to complement the archaeological record to explain the behaviour of prehistoric Mediterranean hunter gatherer subsistence and settlement systems; or to other historic social groupings/communities.[16] In this respect I assume that human sociobiology will attempt to preempt some domains of existing social theory, which are incompatible with sociobiology. On this issue see the book by Alexander Rosenberg: *Sociobiology and the preemption of social science,* Rosenberg (1981).

What is the relation of genes to culture?

The relationship between culture and genetics is such that they are both necessary. They both require each other. Sociobiology sets the framework, and orthodox theories of culture must say at least how the universal structures of sociobiology are realized. The relation may be expressed as follows: Culture without sociobiology is blind, and sociobiology without culture is empty.

And it is a natural requirement of archaeological methodology that not only must the theories of culture and sociobiology be consistent with archaeological orthodoxy; archaeological orthodoxy must be consistent with, as yet unfalsified, bio-science accounts of culture and sociobiology.

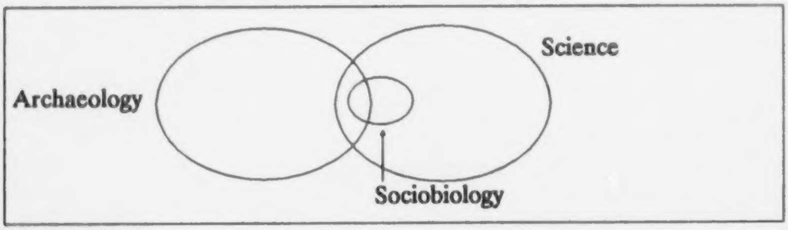

Diagram [a]

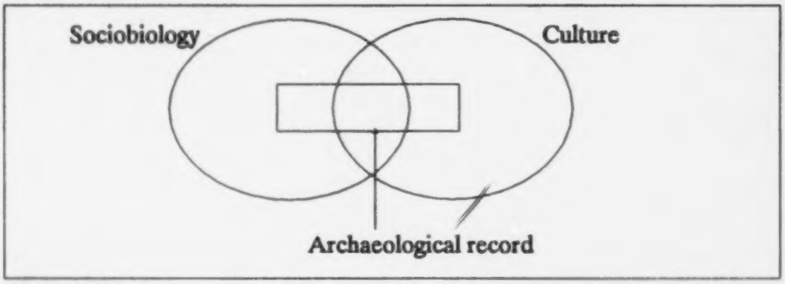

Diagram [b]

It was easy to claim above that, culture without sociobiology is blind, and sociobiology without culture is empty. What one means by that is at least in part that a culture which negates the fundamental posits of sociobiology is blind, and a sociobiology which is at odds with the historical cultural record is empty, or from a theoretical point of view valueless.

However, the relation between genes and culture, or between the results of animal sociobiology and human sociobiology admits of different stances. Some of these are as follows:

R.L. Trivers holds that "The chimpanzee and the human share about 99.5 per cent of their evolutionary history, yet most human thinkers regard the chimp as a malformed irrelevant oddity while seeing themselves as stepping stones to the Almighty. To the evolutionist this cannot be so. There exists no objective basis on which to elevate one species above another. Chimp and human, lizard and fungus, we have all evolved over some three billion years by a process known as natural selection. Within each species some individuals leave more surviving offspring than others, so that the inheritable traits (genes) of the reproductively successful become more numerous in the next generation. This is natural selection: the non-random differential reproduction of genes. Natural selection has built us, and it is natural selection we must understand if we are to comprehend our own identities.[8]

And according to R.D. Alexander, "A Darwinian model seems to me to be established beyond doubt as an appropriate hypothesis for the background of a significant, if not major, function of the proximate behavioural tendencies and motivations of humans."[9]But, the father of sociobiology, E.O.Wilson is more cautious. He says: "It is part of the conventional wisdom that virtually all cultural variation is phenotypic rather than genetic in origin." And he admits "the genes have given away most of "their Sovereignty'! But Wilson also believes that small genetic effects can have large scale behavioral effects, not only for animals but also for humans.[10]

Maynard-Smith is sceptical. He does not see his work on insects contributing to the understanding of human sociobiology and denies that his ideas about insects or animals may also refer either directly or by analogy to humans (Ruse: 1985: p. 53).

Is sociobiology unfalsifiable?

It is sometimes claimed that sociobiology is unfalsifiable on the grounds that it posits individual (or selfish gene) natural selection (survival of the fittest) to explain selfish behaviour: kin selection to account for altruistic or submissive acts directed at relatives: and, reciprocal altruism to explain apparently unselfish behaviour towards non-related persons. All other behaviour is explained in terms of adaptation or lack of adaptation, so the model is really unfalsifiable.

However, there are certainly potentially falsifiable claims in the hard core, and positive heuristic of the sociobiology SRP.

Falsifiable claims:

(1) Sociobiology claims that there is differential production for all. This could be false. There could be one and only one offspring for all.

(2) Sociobiology claims that differential reproduction will be systematic, - it is not just chance which organism survives and which replicates. The initial conditions and positive heuristic explain why this is so.

The theory of evolution is the necessary consequence of three hypotheses. These are part of the hard core, and can be set up with suitable initial conditions in the positive heuristics as observations which may be tested:

1. There is phenotypic variation; the members of a species do not all look alike, or act alike.
2. There is correlation between parents and offspring.
3. Different phenotypes leave different numbers of offspring in remote generations.

The essential presupposition for genetics is one can abstract characteristics from organisms. Characteristics like brown eyes or a large chin can be explained by reference to the basic laws of genetics. Physiological and biochemical links between genes and characteristics can be given. And these can be brought under known laws of heredity. At any stage the theory, if it is false, can be falsified.

The abstractive process can be extended to behaviour. For example, in animal sociobiology, a list of traits in Drosophilia (fruit-flies) can be selected for, under the control of genes, such as selection for mating preference can be carried out by allowing free mating in a mixture of 2 mutant stocks, and then destroying all hybrid progeny in each generation. After a number of generations you can establish mating preferences. This process could be shown to be resting on false assumptions at any stage. So far no one has.

In human sociobiology there is an established genetic case for schizophrenia. If you have it, it must exist somewhere in your family. If false, this could be shown to be so.

There is a general claim in both animal and human sociobiology that organisms all know their degree of relatedness to other organisms. Many anthropologists have reported acute awareness of relatedness among preliterate cultures. But this claim is also supposed to be true of, say, birds. The mechanisms by which animals can be said to be aware of kinship are in most cases not known. This is similar to ornithologist believing that birds are guided in their migrations, but in many cases do not know the mechanism. Starlings are supposed to be guided by the night sky. They migrate at night. So here is something which can be falsified.As it stands it is part of the hard core and the whole research programme is not abandoned simply because an explanation is not to hand. This is also a case of the theory predicting novel facts.

There is a theoretical continuity between Darwin and sociobiology. Three points are relevant here: (1) Sexual selection in Darwin is something between members of the same species. (2) Darwin refused to follow Wallace into group selective mechanisms. Darwin insisted on individual selection. 3)Darwin wished to apply his principles to Man. The really giant development in bioscience, and sociobiology in the early 20th Century was to develop the theory and techniques which were required to glue together in the hard core Darwinian selection and Mendelian genetics.

From the point of view Lakatos's model for judging a research programme general bio-science (including animal sociobiology and population biology) have a large number of plus points:

(1) It unifies widely disparate areas of study - from bio-geography to embryology.

(2) It leads to worthwhile predictions.

(3) It has no rivals to replace it.

(4) It is metaphysically and materially acceptable in that it tries to explain adaptation through "normal" terrestrial or material events rather than by reference to transcendental posits such as "god".

(5) The hard core assumes that adaptive advantage is a good heuristic guide. If this is so it directs sociobiologists to look for the precise nature of adaptive advantage.

On the minus side the sea of anomalies in which sociobiology flourishes includes some of the following which the negative heuristic holds at bay: (1) suicides - the non survivors; (2) reproductively inactive monks or nuns - the non replicators; and (3) non adaptors - people who do not modify their behaviour in order to survive.

Sociobiological categories

In this section I set out some of the generally accepted descriptions and predictions of human sociobiological theory as conjectures. I then conjecture that these structures can be used to explain social organization, and what is meant by "human nature". I assume the reader is familiar with the three theoretical possibilities that the unit of selection could be the group, or the individual, or selfish bits of chromosome. The present stage of the debate seems to have excluded the first option. I am assuming the viability of the third case, and following Dawkins (1976) calling this the "selfish gene interpretation".

Some of the basic categories, which are fundamental to sociobiology as a research programme, for explaining the behaviour of the extended phenotypic effects of the selfish gene are as follows: *aggression, sex, parenthood, kin selection, parental manipulation*, and *reciprocal altruism*.

There is a vast and quickly growing literature on these categories, and I have space in this paper to present only a small fraction of the available results. The interested reader is referred to Michael Ruse's excellent and clearly written, Sociobiology: Sense or Nonsense? (Ruse, 1985) for an extended account of human sociobiology; and to E.O. Wilson's On Human Nature (Wilson, 1975) The Selfish Gene by Richard Dawkins, (Dawkins, 1976) has rapidly become a classic textbook on the subject.

I now proceed to conjecture that the models of human sociobiological behaviour which are used as explanatory structures within the categories mentioned above can be extended to explain contemporary human social behaviour, and our concept of human nature. What are these models, and what can they tell us about expected behaviour patterns? In presenting the models I shall closely follow the exposition given in Dawkins (1976) and Ruse (1985).

> *Aggression:* A great deal of aggression in both humans and animals is adaptive. It is directed toward acquisition of limited or scarce resources. As Wilson says "non-sexual aggression... within a speciesserves primarily as a form of competition for environmental resources including especially food and shelter" (Wilson, 1975: 243).

Aggression in both humans and animals is caused by a number of factors, most particularly by the actions of strangers. One doesn't have to take a position on whether aggression is innate or learned. The function is important. When individuals have their food supply or living space or reproductive strategy threatened, they respond aggressively. And, with some qualification, it is true that human and animal responses to strangers are similar. We distinguish between us and them; family, friends, colleagues and others; people who look and behave like us and anyone who dresses, speaks, and behaves differently. According to a leading sociobiologist, R. D. Alexander, the capacity to live socially together is the product of aggression. Two of the bad effects of social living are diseases and parasites. They conspire to reduce the size of a band/group. So there has to be a good reason for large groups/bands. Alexander has suggested that aggression, sociability and intelligence are adaptations to an environment with predators competing for relatively scarce resources.

> I suggest that, at an early stage, predators became chiefly responsible for forcing men to live in groups, and that those predators were not other species, but larger stronger groups of men.[11]

The alternative explanation that prehistoric man needed to band together to secure larger or more powerful prey is not as convincing as Alexander's. Smaller groups could have been just as effective.

SEX: Most sociobiologists think that theories of sexual behaviour first conjectured for animals can be applied to humans. In both animal and human sociobiology mating and sexual behaviour is construed as a strategy where each sexually active adult tries to maximize his/her own replicational success.

Even though it is true that human life is determined (and in many cases "overdetermined") by culture, human sexual behaviour despite cultural determinants is not falsifier of the general sociobiological predictions. In the case of the female she must contend with a 9 month pregnancy and approximately 12 years of child rearing. After impregnation she is literally left holding the baby. So her best strategy is, either, according to Dawkins (1976), to offer"domestic bliss" or to choose "he-men." The "domestic bliss" strategy is to attempt to get the male to provide some parental care. The female can offer the male sexual favours before impregnation, with the promise of more in return for "domestic bliss." Females who are very attractive can offer less, and unattractive females probably have to raise the threshold of sexual giving. Wilson points out that the human female is sexually receptive at times other than the periods of ovulation. This is connected to the fact that she needs help for 12 years or more.

The "he-man" strategy is to marry "upmarket", i.e., up the socio-economic scale. To do so is to ally yourself with economic success, and to choose genes from the sort of mate whose genes maximize the potential replicational success of the progeny. There is some kind of symmetry in the "he-man" strategy. The most desirable females get to choose the most successful males; and the most successful males get to choose from the pool of the most attractive and desirable females.

In the case of the male he can impregnate with little effort and his inventory of sperm is large. Whereas the average female can bring approximately 15 eggs out of about 400 produced during her lifetime to maturity, the male inventory of sperm over a lifetime is in excess of 1.560,000,000,000 and of those about

60% are usable for impregnation. The New Scientist (1988) reported on research in Manchester which found that about 40% of the average ejaculate is used to form a barrier over the entrance to the womb (cervix) to stop other sperm entering while the 60% of usable sperm attempt fertilization.

So males ought to try and invest as little time and effort as they can get away with. Females have to try to prevent them from doing so by the "domestic bliss" or "he-man" strategy. There should be a selective adaptive disposition towards adultery, where males can have reproductive success, but where the females have other partners who will help raise the offspring. And we should also expect to find a selective adaptive disposition towards not being a cuckold. According to Trivers archaeological data do not falsify this prediction.[12] Trivers shows that where the cause was known, the overwhelming cause of fatal fightsamong Bushmen was adultery. The same is true of certain Eskimo tribes, and Australian aborigines.

In the female there is selective adaptive behavior which shuns adultery. It runs counter to either the "domestic bliss" or "he-man" strategy to allow impregnation by random males not prepared to invest time and money in offspring.

So the male is thwarted by the already mated females pursuing their best strategies. It is therefore easier for the male to get sexual favors from his mate, who will expect help with the offspring in return for sexual favours. The male also has an interest in a period of time to be spent with the female of his choice, before impregnation. If all these push-pull forces commit him to some care of the offspring then it is in his interest to be relatively sure that it is his own gene that he is committed to caring for. Here the male strategy complements the female strategy "coyness", where she is ready to offer attractions before impregnation, but not too eager to start breeding before she has established that the male has the potential for some child care and faithfulness to her (Ruse, 1985: 57-61)

Kin selection

Sociobiologists believe that their theory of kin selection is well corroborated. They argue that kin selection has been a fundamental factor in producing human nature. It was Radcliffe-Brown who pointed out that many pre-literate societies evinced a remarkable understanding of kin. In these societies it was standard to be able to work out relationships in great detail.

> Man is aware, to an extraordinary degree, of differences in his relationship to the other men with whom he lives (Alexander, 1971: 117)

Pre-literate societies allow their social behaviour to be governed by kinship relationships. In general, you would expect more from closer relatives than from distant ones, or non-relatives. If kin selection was a fundamental causal factor in the development of the human genotype then this is what you should expect. There is evidence to suggest that kin selection was historically important. Alexander has argued that kin selection was involved in violent competition. His theory requires that:

> ... war ... (is) ... waged in some relationship to degrees of genetic difference and raises the question of the selective value and background of assisting one's closer relatives at the expense of non-relatives or distant relatives. In any species engaging in the more violent kinds of intraspecific competition, ability to recognize and spare close relatives would be highly favoured (Alexander, 1971: 117).

He goes on to say:

> In some modern hunting-gathering peoples still living in small groups, and in which inter- tribal aggression is prevalent, the extent and nature of such knowledge [of kin] has amazed anthropologists more than any other of their attributes (loc. cit.).

It might be objected that kinship systems on occasion do not work in the way that predicted by sociobiology. According to the positive heuristic of the sociobiological SRP the position is that any conflict between their predictions and observational basic statements will be apparent and not actual. An example of an anomaly which has been turned into a successful prediction is the mother's brother phenomenon.

The data for some societies shows that responsibility for male parental care is delegated to the mother's brother rather than the father. This is a counter-example since sociobiology predicts that the dispenser of male parental benefits should be the biological father. Why should males support 25% relatives rather than 50% relatives? Alexander (1977a) suggests that in such societies there is doubt about who the biological father is. In such cases mother's brother's care is just what kin selection would predict. A dubious 50% relation is deleted in favour of an assumed 25% relationship. Alexander says:

> To summarize, whenever general living conditions or other society-wide circumstances lead to a general lowering of confidence of paternity, a man's sister's offspring, alone among all possible nephews and nieces, canbecome his closest relatives in the next generation. In consequence, so long as adult brothers and sisters tend to remain in sufficient social proximity that men are capable of assisting their sister's offspring, a general society-wide lowering of confidence of paternity is predicted on grounds of kin selection to lead to a society-wide prominence, or institutionalization, of mother's brother as an appropriate male dispenser

> of parental benefits. This appears to be exactly what happens. The evidence indicates that lowered confidence of paternity, fragility of marriage bonds matrilineality, and shifts toward a prominence of mother's brother go together, in a pattern almost dramatically consistent with a Darwinian model of human sociality (Alexander, 1977a: 17).

Another prima facie counter example involves cousins. Some societies distinguish between "parallel-cousins" and "cross-cousins". The former are progeny of siblings of the same sex, and the latter, offspring of siblings of the opposite sex. And often parallel-cousins are regarded as being closer relatives than cross-cousins. However, according to sociobiological theory the preference ought to be the same since the degree of relatedness between parallel-cousins and cross-cousins is one-eighth. But Alexander claims that there is a significant correlation between societies which make these cousin distinctions, and societies where parallel-cousins tend to be closer biological relatives than cross-cousins. He claims that the kin closeness is due to the data context of polygynous societies where males have more than one wife, often sisters. In these instances parallel-cousins are frequently half-siblings. Sociobiological theory is again vindicated since:

> ... a symmetrical treatment of cousins is very strongly concentrated in precisely the kinds of polygynous societies in which it is predicted to occur from an inclusive fitness model, and least frequent in the kind of monogamous society where that is expected (Alexander, 1977a: 18).

Sociobiology has a great deal more to say about kinship, but I do not have room for it here.

Parenthood

The dominant theme here is that of "reproductive self-interest." And the question is: What strategy will best maximize my genes in future generations? There are different strategies depending on the number of children involved.

The one child case: Conflict will arise between the parent's interests and the child's interests. At some stage it is better to invest less in an only child and more in a second child. But from the point of view of an only child it is best to demand as much as it can get. The parent and child are in some senses genetic rivals.

Altruistic behaviour by an offspring might benefit a parent more than the offspring. When an individual helps a first cousin it is helping an individual related by 12.5% to itself. However, that individual is related by 25% to the parent. In some cases it is in the parents interest to push the offspring to altruism, but not in offsprings interest to comply.

A fundamental conflict is expected during socialization in the two or more children case over the egoistic/selfish and altruistic/non-selfish impulses of the offspring. Parents are expected to socialize their offspring to act more altruistically than they would otherwise act, that is to act in such a way as to benefit the other offspring; and the offspring should resist because they are related genetically by only 50% to other full siblings, but are related by 100% to themselves. The story of Cinderella is a mythical case in point of this kind of phenomenon. As Michael Ruse shows, the teaching of honesty, decency, generosity, is not a question of introducing children to culture, or to civilization, but pushing the children to support the parent's biological or replicational interests.

The conflict is not between the culture of the parent versus the biology of child. It is instead a conflict based on the sociobiology of the parent versus the sociobiology of the child (Ruse, 1985: 61-63).

A hunter-gatherer model for the prehistoric Mediterranean

Any success which sociobiology has when applied to archaeology is, from the point of view of theory, dependent on generating predictions of expected behaviour in the present which can be retrodicted into the past. Arthur S. Keene in his "Economic optimization models and the study of hunter - gatherer subsistence settlement systems makes certain assumptions:[6]

Assumption 1: Economic activities among Hunter-Gatherer are organized. (Sociobiological activities are organized.)

Assumption 2: The primary goal among Hunter-Gatherer is to provide the basic nutritive and other raw materials necessary for the survival of the population. (The interests of survival, adaptation, and replication are preeminent.)

Assumption 3: When faced with a choice between two resources of equal utility, the one of lower cost will be chosen. Economic behaviour is both satisfying and optimizing. (Sociobiological behaviour is satisfying and optimizing.) The remarks in parenthesis are a reinterpretation of Keene's assumptions on the sociobiological model).

Keene uses these assumptions and his deductivist model to account for behaviour in a Netsilik social economy both pre-rifle and post-rifle. The model accounts for usage of caribou, musk-ox, ringed seal, bearded seal, polar bear fish, birds etc. Of course it is a simplified model. But it is potentially falsifiable. It cannot be overstressed from the point of theory how important in this context this latter fact is.

Sociobiologically we would make different assumptions to arrive at an efficiency optimization model of gene-replication. These would involve the question of the optimum number of babies given survival conditions. Religion and social organization would have to be arranged to optimize and maximize this central concern of sociobiology.

The general problem about making cultural predictions or retrodictions, which is referred to as the problem of the hermeneutic circle. The problem can be stated as follows: a theorist can only understand a whole culture by understanding the "discourse" or "meanings" in the social interactions. And to understand the whole of a culture you need to understand some part of it. Understanding some part of it requires a knowledge of the terms and usage of that part of the language which capture the meanings of the part of the culture you wish to understand. But in the case of the prehistoric Mediterranean it is clearly impossible to understand the language of e.g. the prehistoric Maltese. And since you cannot break into the hermeneutic circle you cannot understand the culture.

Furthermore, the theorists who appeal to the problem in a sceptical fashion are usually theorists who assume that there is just one general concept of "human nature", but that in any particular culture what is meant by human nature is to be understood by the particular mix of psychology, mythology, religion, politics and family life which is current at some date in that culture. So again, we would not understand what prehistoric Maltese meant by human nature because of our ignorance of the language which was used at the time.

If the only way into a culture, or if the only understanding of human nature were an internal linguistic one then no important cultural projections could be made back into the prehistory of the Mediterranean. And we would be in the dark about human nature.

It should be clear that the model of sociobiology adjoined to Keene's model of hunter-gatherer behaviour can circumvent the hermeneutic circle problem. Sociobiology gives us an account of human nature which bypasses most of the assumptions made by the statement of the hermeneutic circle problem.

Concluding unscientific postscript

Archaeology appears to be on the road to becoming a science subject. To become a science it will have to acquire a methodology which is genuinely scientific. I submit that the methodologies of Karl Popper and Imre Lakatos will be both useful and necessary if the subject of archaeology is to become a science. The theories and hypotheses of archaeology must be open to refutation and falsification as intellectual honesty in the sciences require. One of the virtues of the Popper approach is that it can also cope with many parts of archaeology which for the moment will remain outside the domain of science. An overall archaeological perspective will have many sub-components: linguistic, archaeological, ethnographic, art-historical, religious, philosophical, and perian method obviously applies; techniques derived from physics, chemistry, engineering, economics, demography, environmental ecology, and mathematics. It seems best to regard the most accessible parts of the non-scientific sub-components as sets of hypotheses and theories which are conjectured for potential refutation. Alternative methodologies for Archaeology should be seen in the perspective of Popper - Lakatos evolutionary epistemology. The fittest methodologies will survive, because they adapt. The others will not leave copies of themselves in remote generations.

Notes

* "The selfish gene in the prehistoric Mediterranean: method in archaeology" paper read to the Second International Conference on the Archaeology of the Prehistoric Mediterranean, at the University of Malta, Valletta, Malta, December 19-22, 1988. This paper is forthcoming in the proceedings of the conference which are to be published later this year. For a further application of sociobiology to the archaeology of the prehistoric Mediterranean see also, Van Straaten, Zak, Philosophical paradigms of fertility cult behaviour, pp 31-41, in *Archaeology and fertility cult in the ancient Mediterranean,* (ed.) A. Bonnano, Gruner, Amsterdam 1986.

1. There are numerous footnotes in the Lakatos text which I have, for lack of space, not reproduced here. The interested reader may wish to pursue them. In one of the footnotes he points out that; "The actual hard core of a programme does not actually emerge fully armed like Athene from the head of Zeus. It develops slowly, by a long, preliminary process of trial and error. In this paper this process is not discussed." And in other footnotes he refers to text where he refines the concepts of "counter-example" and "anomaly".

2. The equivalent conjecture for theoretical physics is given in: Hawking, S. *Is the end in sight for theoretical physics?* Cambridge University Press, 1980 p1.

6. Arthur S. Keene "Economic optimization models and the study of hunter-gatherer subsistence settlement systems, pp. 369-404 of C. Renfrew and K.L. Cooke (eds.) *Transformations: Mathematical approaches to culture change,* N.Y. Academic Press, 1979.

7. C. Renfrew "Transformations" p 3ff and in the Introduction of (C. Renfrew and K.L. Cooke: 1979).

8. Trivers R.L. Foreword to Dawkins, 1976: v.

9. Alexander, R.D. "Natural Selection and the analysis of human sociality" in C.C. Goulden (ed.) *Changing scenes in natural science,* Philadelphia Academy of Natural Sciences, Philadelphia, 1977.

10. Wilson, E.O. 1975: 550.

11. Alexander, R.D. p. 116 "The search for an evolutionary philosophy." Proc. of the Royal Society, Victoria, Australia 84; 1971 pp.99-120).

12. Trivers R.L. "parental investment and sexual selection," in B. Campbell (ed.) Sexual Selection and the Descent of Man 1871-1971, p. 149 Chicago, Aldine 1972.

13. Three recent critics of this model are Lewis R. Binford "Meaning, inference and the material record" pp. 160-163: Robert Whallon Comments on"explanation" pp. 155-158): and Colin Renfrew, "Socio-economic change in ranked societies" (pp. 1-8) in (Renfrew and Shennan, 1982). For a discussion of similar issues in methodology see: Plog, Fred. "Laws, systems of law, and the explanation of observed variation": and Binford, Lewis R." Interassemblage variability-the Mousterian and the "functional" argument", in Renfrew, Colin (ed) *The explanation of culture change, models in prehistory,* G. Duckworth, London 1973.

14. For a deft analysis of the practical problems in connecting science based archaeology to so-called classical archaeology see Pollard, Mark. Archaeology in Ruins? forthcoming in *New Scientist,* January 21, 1989.

15. Lowenstein and Zihlman 1988: 56-59.

17. Lowenstein and Zihlman 1988: 57

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no clear indication of who, if anyone,

This book brings together a selection of papers presented at a conference *Knowledge and Method: A Conference on the Philosophy and Methodology of the Human Sciences*, which was organized by the Human Sciences Research Council in Pretoria in January 1989. These papers are a reflection of the great diversity in the various disciplines of the human sciences. Also the authors represent very different conceptual frameworks and paradigms. The issues vary from philosophical and meta-theoretical to concrete and practical concerns of research.

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